



No. 12. Ep. 26.

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From the Author

ON TYPHOID FEVER

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ON

TYPHOID FEVER

BY

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IN AUSTRALIA," ETC.

MELBOURNE
GEORGE ROBERTSON


MDCCCLXXIV.

369.

MELBOURNE :

STILLWELL AND KNIGHT, PRINTERS.

“Owing to the unhappy and calamitous division of the Profession into
“medicine and surgery, arose those corporate distinctions that have done
“so much to retard the progress of science in these countries ; exclusive
“schools of surgery sprang up, and consequently, a large and increasing
“number of young men were educated without having ever seen a case of
“fever. They were educated in surgery ; they were educated in a surgical
“ward, and were sent forth naturally advocates of inflammation, because
“they had seen nothing else ; and thus, ill prepared, they went forth to
“combat fever—that disease which numbers more victims than any other—
“over the wide dominions of the British Crown, in America, in the West
“Indies, in Asia, in Africa. These men in hundreds—I may say
“thousands—went out ignorant of the fearful enemy they had to encounter,
“and trusting in the teachings which compared the ordinary phenomena
“of ophthalmia, or those of the healing of an incised wound, or those of
“the cicatrisation of an ulcer, with the symptoms of that terrific group
“of diseases which embraces the plague, the yellow fever, the bilious
“remittent, the malignant ague, and the typhus fever.” * * “Conditions
“of the system which required wine, bark, stimulants, careful nutrition,
“were met by the lancet, by leeches, and by starvation.”—*William Stokes.*



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ON

TYPHOID FEVER

“The origin of epidemics is one of the most obscure and difficult subjects in the whole range of physical inquiry.”—*William Stokes*.

“Contagion is as important an element in medicine as gravity in mechanics or electricity in chemistry.”—*Robert Williams*.

“The subject of contagious disease still offers a spacious field for discovery.”—*Henry Holland*.

ON receiving a letter from the Central Board of Health, requesting me to inquire into the extent and cause of the epidemic of typhoid fever that prevailed here last autumn, I wrote to all the medical men of the city and suburbs, inviting their co-operation. The following questions, suggested by the president, indicated the points on which the board desired to obtain information:—Were the cases of fever true typhoid? Were they more numerous than usual at corresponding periods of former years? or more severe and fatal? Were there any conditions common to the affected localities to suggest a general cause of fever? Did any personal

circumstance make the sick prone to fever? Were there any proofs of contagion? Had any seasonal influence appeared to generate the specific poison which is believed to act as the exciting cause of fever? Or had domestic customs in the mode of milk or water supply spread fever? I also expressed a wish for an opportunity to visit and inspect along with those addressed the places where fever was rife.

To enable a full comparison to be drawn between the present and past years, and to make the answer to the second question complete, statistical records were obtained for fourteen years, of deaths from fever in Melbourne and suburbs; and also, for the same period, of the number of cases of fever admitted into the Melbourne Hospital, with the number of deaths from fever. To find, moreover, how far seasonal peculiarities influenced fever, an elaborate set of meteorological tables has been compiled at the Observatory. The information thus obtained can hardly fail to be of much use in the future study of epidemiology. It is a pleasure to record how readily the returns were given, no pains having been spared to make them complete.

The first point to decide is whether the cases of fever had the characteristic symptoms of typhoid fever. This should be explicit, for any doubt would invalidate all that follows. That fever of some kind has prevailed, with varying severity, especially during autumn, is shown by the testimony of medical men who supply the items to the official statist. From those records, as well as from the case-books of the hospital, and a few excellent papers in the *Australian Medical Journal*, the conclusion may be drawn that the fever usually occurring in this community is typhoid, enteric, or pythogenic fever, identical with the autumnal or fall fever of America, the abdominal typhus of Germany, the *dothiènentérite* of the French, and, of course, with the fever of the same names in Great Britain. By whatever synonym known, it is here, as elsewhere over the world, the same specific fever. It has justly been remarked that our "complex fever nomenclature is puzzling to the student, and misleads the public mind."¹

To form an idea how far this difference of

¹ Watson's *Lectures*, vol. ii. p. 864, 1874. Dr. Murchison enumerates no fewer than 100 synonyms of typhoid fever.

designation is carried, it is only necessary to glance at the various terms used by different medical men to specify the cause of death from this fever for four months of the present year, as in the subjoined list, here inserted for the purpose:—

MANNER OF RETURN OF DEATHS FROM FEVER DURING
THE MONTHS OF—

March.

Typhoid fever	15
Typhus fever	1
Continued fever	1
Gastric fever...	4
Enteric fever	2
Colonial fever	1
Fever and diarrhœa...	1
Low fever	2
Typhoid fever, melæna	1
					<hr/> 28

April.

Typhoid fever	21
Bilious fever...	1
Colonial fever	4
Fever and vomiting...	1
Fever...	1
Relapsing fever	1
Low fever	4
Exhaustion from typhus fever	1
Enteric fever	3
					<hr/> 37

May.

Typhoid fever	15
Colonial fever	4
Continued fever	1
Fever and serous effusion of brain	1
Fever and congestion of lungs	1
Typhoid fever and congestion of brain	1
Low fever and congestion of brain	1
Febricula	1
Gastric fever...	1
Enteric fever...	2
Enteric and rheumatic fever	1
Pyæmia and typhoid fever...	1
					<hr/> 30 <hr/>

June.

Typhoid fever	7
Colonial fever	1
Continued fever	1
Low fever	3
Intermittent fever	1
Remittent fever	1
Enteric fever...	1
Enteric perforation of intestines, peritonitis	1
					<hr/> 16 <hr/>

It is unnecessary to state that no such varieties of fever exist in nature. The various terms merely denote so many ideas of what is the fittest name to use in describing the same specific fever.

The various theories which affirm that the different forms of continued fever — typhus, typhoid, and relapsing fever—are only varieties of a species, derive no support from observation of fevers here, because here neither typhus nor relapsing fevers occur. On several occasions typhus has been imported, but it has no more been allowed to spread by contagion than has small-pox. It is denied by some that typhus ever was seen in the colony, there being, it is said, “no evidence of any vessel having imported it.” But I myself am well acquainted with all the particulars of one ship that brought typhus, and spread it widely over the goldfields, in 1852.¹ On another occasion I traced a con-

¹ The case of this ship is cited by Mr. Herbert Spencer in his *Essays: Scientific, &c.*, as proving the spontaneous origin of fever in a crowded ship. This inference was erroneous. The contagion came from fever beds in Glasgow. The first man was ill while the vessel lay at anchor in the Mersey, before he had been one day on board, or before spontaneous genesis of fever was possible. The patient, an elderly Highlander, had lodged two weeks in Glasgow, on his way to join the ship. I was fellow-passenger with him and six hundred other emigrants, in a small steam-packet from Glasgow to Liverpool. The moment the fever showed itself I, as surgeon-superintendent, tried to land the man at the Birkenhead

nected series of seven cases of typhus fever from one case landed from ship-board.

This question of the identity of fever was debated at the Medical Society here, when the

depôt; but my effort was over-ruled by the port medical authorities, and the ship went to sea with the focus of contagion to thin out her living freight, and puzzle speculative biologists with specious theories about the cause of it. The man died on the fourteenth day. The next person attacked was a man in the adjoining berth, and he too died on the fourteenth day. Fever then slowly spread from one to another, with no outburst amongst a number, as it would have burst out had the poison been bred in foulness of over-crowding. Over a thousand souls were on board of a 1400 ton ship, with two decks, making isolation impossible. By strenuous efforts the fever was kept in check so far as to enable every one to go on deck when the pilot boarded at the Heads. There was no health officer; the ship came on to Hobson's Bay, and was placed in quarantine, *not* for typhus fever, but for whooping-cough!! From that contagious disease a hundred and eighty children were at one period of the voyage ill. There were in all three hundred children, sixty-one under a year old, with twenty-one added by births on the voyage, and all these infants had whooping-cough, from one child embarked with it. A relieving ship took off fever convalescents, leaving the healthy to sicken at their leisure, causing more fever and deaths than on the voyage. Many who went on shore with fever incubating spread contagion far and wide on the gold-fields. As the ship lay at anchor off the Red Bluff, ventilation and activity were less, people grew weary and dejected, and fever spread rapidly. After the ship sailed for India some of the crew died. The history of that

gentleman who introduced the subject, said he had seen typhus in the early days of the gold era, but believed it had died out, giving place to undoubted enteric fever.¹ He thought typhus must have been engendered by the mode of

voyage showed how contagion is carried from old to new countries; that people who have not had fever are as susceptible here as in the old fever-familiar towns; that fermentible blood ferments in this sunny clime as in the murky air of Glasgow vennels; and that typhus virus is not "roasted out of virulence in the tropics." The facts of this classic voyage, however they may justify his charge of Governmental mismanagement, give not a tittle of support to Mr. Spencer's evolution hypothesis. If all his data be equally fallacious, his biological deductions are as baseless as the metaphysic to which his philosophy is antagonistic. Whether abnormal conditions of life, squalor, starvation, crowding, foul air, generate afresh the material cause of fever, or whether the morbid poison must pass from sick to sound always, will remain an enigma to philosophers who take no pains to observe, or to obtain true accounts from those who do observe, such facts as were daily noted by myself during the memorable fever voyage of the *Wanata*. The experience gained during a residence in a fever hospital, where points of diagnosis raised by Dr. Perry and by Dr. A. P. Stewart were daily tested, long ere they were touched by Sir William Jenner, made what and how to observe in fever familiar, and enabled me then to state that had no person suffering from typhus, and no child ill of whooping-cough, come on board, neither disease would have appeared during the voyage.

¹ *Aust. Med. Jour.*, June 1869.

living at the goldfields, because "there was no evidence of typhus importation." His opinion deserves mention, because it was well argued, and because the reply which I received from the same gentleman to the first question relating to the recent epidemic, was that "during the past few months there had been many cases of bilious remittent, but not one with the characteristic symptoms of enteric fever." "These remittents," he added, "have not been more prevalent this season than at corresponding periods of former years, but they have been of more than usual severity, being more prolonged in duration, and the prostration more marked, and the remission less pronounced." That correspondent thus appeared to think that, while in the late epidemic there was much remittent fever, there were but few cases of true typhoid. The phrase "bilious remittent fever" is a synonym for relapsing fever;¹ and since there never has been an instance of that famine fever in this colony, the use of the term is confusing.

¹ Murchison, on *Continued Fever*, p. 309. Dunglison's *Med. Dict.*, p. 893, word "Relapse."

In a paper in the *Aust. Med. Jour.*, Aug. 1867, the clearest evidence is given that fever, as seen at Sandhurst, is typhoid; the symptoms, anatomical signs, and thermometrical charts, proving its identity. One remark of the author's is especially important—that “young children suffer greatly from what are usually called diarrhœa, dysentery, infantile remittent, and muco-enteritis; and he had no doubt that many of the cases of the two former, and the majority of the latter, are really enteric fever.” Of the truth of this I am convinced, and believe the mistake in diagnosis to be the cause of many errors of practice. This point is urged, because one answer to the question as to the type of fever, returned by one of the oldest of our practitioners, was that he “had seen many cases of infantile remittent fever, but none of typhoid.” As has been truly observed, “accounts of the *post mortem* appearances of children dying from these diseases are much wanted, and would be very valuable;” and it is necessary that in future every inducement should be held out to medical men to make them; so that, with clearer views of pathology, practice may become

more specific. It will also add to precision if we discard the term infantile remittent fever, and adopt that of typhoid. This change of nomenclature is made by Dr. Harley,¹ and has been adopted, in agreement with many French writers, by Dr. West in his famous treatise on the diseases of children.² The term used to signify a special disease is of little account so long as it precisely denotes the disease signified, but here different terms have been wrongly employed to suggest different diseases, when in reality there is identity. In this colony, where so much has been said and written about children's maladies, the point deserves the utmost attention.

It may as well be here remarked that I have retained the expression typhoid fever in preference to adopting any other term which has, in recent years, been suggested as a substitute; because the phrase is generally well understood to convey a definite meaning, without implying any theoretical or hypothetical inference as to the external cause of the fever, or of its rela-

¹ Reynolds's *Syst. Med.*, vol. i. p. 558.

² *Diseases of Infancy*, 6th ed. p. 771, 1874.

tions as symptomatic of some anatomical lesion. In short I have employed it for reasons precisely similar to those that induced M. Louis to prefer it:—" *Au moins à peu près exempte d'inconvénients.*"¹ The adjective *Pythogenic* cannot be consistently employed, unless the cause be also admitted to arise from decomposing organic matter; which is a theory as yet to be proved.

By whichever term described it is evident that the fever usually met with at Sandhurst is true typhoid fever, and ample proof will be given that the fever so common in Melbourne is exactly of the same type.

Among the first to confirm this opinion, that the recently prevalent fever was true typhoid, may be given the reply of a careful observer, stating that he had during the last autumn several cases of typhoid fever under his care. This physician has, with commendable pains, collated the cases that he had under his charge in the Melbourne Hospital, as well as in private practice, for three months—February, March, and April—in the years 1872-3-4, as follows:—

¹ *De Fièvre Typhoïde*, tome premier.

IN HOSPITAL PRACTICE.

	1872.			1873.			1874.		
	Males.	Females.	TOTAL.	Males.	Females.	TOTAL.	Males.	Females.	TOTAL.
—									
February ...	3	—	3	—	—	—	2	2	4
March ...	4	3	7	3	—	3	4	3	7
April ...	5	3	8	2	2	4	1	5	6
	12	6	18	5	2	7	7	10	17
Deaths ...	1	—	1	—	1	1	—	2	2

IN PRIVATE PRACTICE.

	1872.			1873.			1874.		
	Males.	Females.	TOTAL.	Males.	Females.	TOTAL.	Males.	Females.	TOTAL.
February ...	2	2	4	—	—	—	1	2	3
March ...	1	1	2	—	—	—	3	—	3
April ...	2	—	2	2	2	4	2	—	2
	5	3	8	2	2	4	6	2	8
Deaths ...	—	*2	2	—	—	—	—	1	1

* These two fatal cases were seen in consultation.

He likewise adds a statement of his opinion that the frequent occurrence of typhoid fever at this season—autumn—is mainly attributable to the facts that heat and moisture favour putrefaction, and that the atmosphere may thus become impregnated with emanations from decomposing excreta.

This view of the etiology of typhoid nearly agrees with Dr. Murchison's famous pythogenic theory; but as the question will be again discussed in connexion with meteorological data, it is left till then. Suffice it here to adduce trustworthy testimony as to the type of fever lately epidemic.

There is also evidence from the reports of the Melbourne Hospital that the fever was true typhoid. If a correct diagnosis may be looked for anywhere, surely it may be found there. In Table I. the number of cases of typhoid fever treated in the hospital is given, with the number of deaths, for fourteen years—from 1860 till 1874 as far as it has gone—showing that 559 cases were so named, of which 140 were fatal.

♦

TABLE I.

RETURN SHOWING THE NUMBER OF CASES OF TYPHOID FEVER TREATED IN THE MELBOURNE
 HOSPITAL DURING THE YEARS FROM 1866 TO THE END OF MAY, 1874, WITH THE DEATHS
 FOR THE SAME PERIOD.

Years.	January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total Deaths.	
	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Cases treated.	Deaths.	Total Cases treated.			
1860	1	1	1	1		
1861	2	3	...		
1862	1	1	1	1	7	...		
1863	2	1	1	2	1	2	1	2	9	3		
1864	5	1	3	1	1	1	20	6		
1865	1	2	5	2	3	7	3	19	9		
1866	1	1	9	3	10	3	8	3	19	2	9	2	10	1	2	...	1	1	70	16		
1867	4	2	2	...	5	2	8	...	15	2	4	1	3	1	1	44	9		
1868	3	7	1	3	2	11	4	1	...	1	...	1	37	8		
1869	4	1	5	...	8	...	8	...	15	1	5	2	2	1	1	2	1	4	...	56	9		
1870	6	2	1	11	24	5	31	5	31	5	8	2	5	2	1	1	1	...	1	3	97	19		
1871	7	3	8	1	9	1	7	2	7	...	8	3	5	1	...	1	2	1	54	12		
1872	5	1	7	...	11	1	7	2	9	1	4	2	2	1	2	1	4	1	1	...	51	9		
1873	2	...	3	7	2	6	2	7	1	1	1	4	2	5	43	13	
1874	7	3	8	7	12	6	12	4	9	6	48	26		
Total Cases treated	43	...	51	...	86	...	96	...	135	...	48	...	29	...	14	...	10	...	12	...	16	...	19	...	559	...
Total Deaths	16	...	14	...	18	...	21	...	27	...	12	...	7	...	4	...	3	...	5	...	5	...	8	...	140

The total number of fever cases during those years is 2,816, with 313 deaths (*see* Table II.), and it becomes a question, what other fever beside the 559 cases of typhoid occurred ?

TABLE II.

TOTAL CASES OF FEVER TREATED FROM 1860 TO 1873 IN
THE MELBOURNE HOSPITAL.

YEAR.	TOTAL TREATED.	DEATHS.
1860	425	35
1861	450	36
1862	192	22
1863	139	4
1864	164	23
1865	193	30
1866	194	24
1867	182	22
1868	128	17
1869	160	16
1870	211	29
1871	112	19
1872	140	14
1873	126	22
Total - -	2,816	313

It would appear that few cases of fever were called "typhoid" in the hospital before

1864. For instance, in 1860, there were in all 425 cases of fever, and 35 deaths, yet only one of the number, a case in June, was so classified; while in 1861, of 450 cases of fever, with 35 deaths, only 3, occurring in August and September, were set down as typhoid (*see* Tables I. and II.); so that either the large number of cases of fever reported during those two years were really not typhoid fever or else the hospital physicians of those days did not recognise the fever. The years 1860, 1866, and, as far as it has gone, 1874, have shown the largest mortality in proportion to population from fever. About 1860 the question of diagnosis and type of prevailing fever excited keen debate within the hospital, and both the eruption of rose-spots and intestinal ulcers, the ulcers of Peyer's patches, as the most specific of the anatomical lesions of typhoid fever, were frequently pointed out by those who did recognise its true nature. At length, coming down through later years to the present, the character of the fever that pays its periodical visit every autumn is now almost universally admitted, and as such is recorded.

The fact of there being no longer any doubt about the diagnosis within the hospital is a fair indication that the fever outside is precisely the same typical typhoid.

But, although the diagnosis is thus nearly settled, some few deny the existence amongst us of genuine typhoid, and, as they speak emphatically, it will be well to consider the facts on which they seem to base their opinion. One gentleman states that the fever cases he has lately seen had not the characteristic symptoms of typhoid, were not more prevalent than usual, nor more severe or fatal, and that there was no sign of any common cause of fever; but he adds—"There is a great number of fever cases, all common continued fever, in the lower parts of Collingwood. Applicants of that class are numerous at the Collingwood Free Dispensary." Another gentleman connected with the same charity states that he has "had very few cases of true typhoid fever, although he has had each year several hundred cases of fever, ill for a longer or shorter period, with a very small mortality; and adds—"I have had some lately with spots varying in size from that of a three-

penny to a shilling (*taches bleuâtres?*) over the body, limbs, and face, but the other usual symptoms of typhoid were absent."

It would be desirable for those who have a great number of fever cases coming under observation, but who deny its typhoid type, to reconsider the question of diagnosis, defining exactly what is meant by common continued fever. Murchison remarks that—"Many cases are designated simple fever or febricula which are in reality mild or abortive cases of typhus or enteric fever." "The typhus and enteric poisons occasionally give rise to symptoms so mild and indefinite, and of so short duration, that an accurate diagnosis is impossible unless well-marked cases of either fever occur in the same house at the same time. Accordingly, the term 'simple fever' has become a refuge for many cases of uncertain character."¹ Although the same high authority also admits that cases of short fever, independent of any specific poison, are occasionally met with in all countries, and in some are common, the more

¹ *Continued Fevers*, p. 678.

probable inference is that the vast majority of cases of simple fever or febricula seen here are aborted cases of typhoid. Lebert and Niemeyer likewise termed those mild cases 'abortive' in preference to 'febricula,' &c., because better expressing the fact of their being only modified, benign, brief forms of typhoid, and not a peculiar variety of fever.¹

In further evidence that the fever is true typhoid may be adduced the experience of a gentleman in an extensive practice, who, in reply to the circular, stated that he had seen nineteen cases of fever during last March, April, May, and June, eighteen of which "exhibited the characteristic symptoms of true typhoid." He found them much more prevalent than at any time during the past five years, and attended with more than usual severity. Several well-marked and severe cases occurred at Footscray. One correspondent mentions having seen many cases "with entire absence of the eruption, but otherwise characteristic." At Emerald Hill typical cases were observed.

¹ Niemeyer, *Pract. Med.*, vol. ii. p. 591.

At Hawthorn cases were reported as typical and far more prevalent. At Richmond fevers were reported as of a mixed character, but more prevalent. At Prahran they were characteristic and more prevalent than usual. At Williamstown cases were occurring during autumn more frequently and severer in type last season than in past years. Four cases were reported by a competent observer at St. Kilda as of genuine typhoid. One of the oldest practitioners of Richmond stated, "There can be no doubt but that the fever (typhoid) in this district has been more frequent, more severe, and also more fatal than it has been for many years." Another residing in Sandridge stated, "No case of typhoid fever has been seen here for many a long day." Thinking that this singular exemption might furnish a clue to the special conditions of the alleged spontaneous origin of typhoid fever in other differently situated localities, I searched the returns to verify the statement, but found it to be inaccurate, as the following deaths from fever under various names, but all denoting typhoid, occurred at

Sandridge in 1871, 1872, 1873, and in the first half of 1874 :—

1871.

Diarrhoea and low fever	1
-------------------------	-----	-----	-----	---

1872.

Bilious fever and congestion of liver	1
---------------------------------------	-----	-----	-----	---

1873.

Colonial fever	1
----------------	-----	-----	-----	---

Gastric fever	1
---------------	-----	-----	-----	---

Gastric fever and enteritis	1
-----------------------------	-----	-----	-----	---

1874.

Gastric fever	2
---------------	-----	-----	-----	---

Thus, instead of none, there were no fewer than seven deaths in three and a half years, representing at least seventy cases of typhoid fever. This mistake affords a fair example of the bad effects of using a mixed nomenclature, by raising in the mind ideas of different fevers, when in reality there is, most certainly, only one specific type with varieties.

It will thus be observed that, making every allowance for the inevitable diversity of medical opinions, there is a preponderance of testimony by men engaged in private practice that the

fever of last autumn was true typhoid, and that it was more prevalent, more severe, and rather more fatal than usual.

This very general acknowledgment of the specific character of the fever might be considered sufficient answer to the first question; yet, to corroborate the conclusion thus drawn from the ready statements of men busy with the daily duties of medical practice, without leisure to elaborate from notes or case-books such as physicians use who enrich medical literature, may be added the evidence obtained from the Alfred Hospital, where the few cases in that institution have been observed and reported in the subjoined comparative tables:—

1873.			
MONTH.	CURED.	DIED.	TOTAL NO. OF CASES.
January ...	4	I	5
February ...	I	I	2
March
April... ..	I	...	I

1874.			
MONTH.	CURED.	DIED.	TOTAL NO. OF CASES.
January	I	I
February
March ...	8	I	9
April... ...	7	I	8

On 21st April, 1873, two cases of typhoid fever were under treatment. On 21st April, 1874, there were six cases.

An inspection of the temperature register for morning and evening would satisfy any one who is at all conversant with the thermometry of fevers, that no other disease save typhoid fever could give similarly decided chart tracings as were got from the cases of typhoid that were treated in the Alfred Hospital. As Dr. Budd has well observed of the intestinal lesions of true typhoid, take away them, and leave the whole body, and no one can tell the history of the fatal fever ; but leave the intestine, and take away all the rest of the body, and no one should have any doubt as to what had been wrong. So

may the same precision be reached during life from carefully noted thermometry; so much so indeed, that no man's opinion as to the diagnosis of typhoid by symptoms during life is acceptable who does not adduce the thermometrical record of his cases. Wherever that and the result testify to the presence of typhoid fever, it may safely be said of a community, what Dr. Murchison has said of a household, that wherever cases of genuine typhoid have occurred, any cases of less pronounced type occurring at the same time in the same neighbourhood may be set down as modified or abortive cases of the same specific fever; and that such is the truth of all those cases of fever, lately named by other terms, amongst us, will probably be accepted by most people. Thus, from Williamstown, it was reported:—"While the instances have been numerous, the type has been comparatively mild." A fine lad, seventeen years of age died, whose case was characterised by intractable diarrhœa, low muttering delirium, rose-red lenticular spots, and profuse hæmorrhage from the bowels. In two others convalescence was reached through

great emaciation and bodily prostration ; while seven cases "could scarcely have been identified with true typhoid fever but for their occurring in the same families, and by their having the invariable abdominal pains accompanying an illness of a week's duration." Here were three well-marked cases of typhoid surrounded by seven abortive cases ; and doubtless similar instances have taken place, but unobserved, in other localities. For example, a gentleman practising at Emerald Hill, stated that he lately had the first case of real typhoid in five years' practice, and was of opinion that "ere many years have elapsed fever will be as prevalent as in the old country." But this will only be because medical men fail to discern the true character of the milder type of cases. As long as physicians persist in regarding as typhoid only such cases as are characterised by a copious eruption, persistent diarrhœa, or by intestinal hæmorrhage, so long will the cause elude discovery, whether it be of spontaneous origin, or a propagated morbid poison.

As far as my own observations go, made upon the score of cases that fell under my notice

during last March, April, and May, the true character of typhoid fever was in every instance unmistakable. In three of them, seen in consultation, my inquiry if there were an eruption of spots was answered by the medical men in attendance in the negative; but, on closer inspection, the spots were not only distinctly visible, but in one fatal case in an unusually copious eruption. The spots either had been overlooked, or, what is more probable, not looked for. Some of the other equally characteristic symptoms were similarly unobserved, though quite as evident. In very few instances in general private practice is the morning and evening temperature of the body taken by the thermometer in typhoid fever. This operation involves a morning and evening visit to the patient, and to this friends may demur; so that the physician either has to be satisfied with a daily call, or, if his zeal incite him to do his duty as it should be done, his labour goes unrequited if his motive be misconstrued. When danger is imminent, no doubt friends alarmed would have the doctor constantly; but, at the onset, when he alone is anxious about the issue,

an extra evening visit raises in the minds of patients, or of relatives, only an idea of superfluous care. Thus are the precious first days of a typhoid fever often allowed to go by, without an exact diagnosis being made, by the only means of making it in a vast number of cases—the thermometer. In the case of a young lady this was the method used several days before either the eruption or diarrhœa took place, and was the means of altering the treatment from a lowering saline to an expectant and supporting system; so that when the eruption and diarrhœa came, the latter was not mistakenly attributed to aperients, as it might otherwise have been. By anticipating other dangerous complications, and directing the treatment, thermometry here saved life. It might have been equally successful in another case, that proved fatal, had the opportune evening visit for taking the temperature been made. The severe prostrating diarrhœa that suddenly set in had not been foreseen. In this case the eruption of rose spots on the twelfth day, diarrhœa of ochrey fluid with a flocculent stratum, great tympanitis, high pulse, dilated

pupil, restlessness, constant desire to leave bed, delirium, and finally dissolution by death beginning at the heart, left not a doubt possible of the nature of the disease. An autopsy, for sanitary reasons, was not made, although the omission is now to be regretted. No inquiry into the cause of the epidemic was then contemplated, and the diagnosis had been exact enough for the requirements of the case itself in determining at least its nature; but a description of the state of the agminated and mesenteric glands would now be of singular service.

Close by the residence of the young lady whose case is above alluded to, a child three years of age was ill with fever and diarrhœa. The mother, seeing me examining the child's body daily for an eruption, began herself to watch, and on the eighth day saw three lenticular spots. From day to day fresh spots came as the former disappeared. This patient recovered satisfactorily; but, had it not been for the daily inspection of the eruption, the mother would have considered her child ill only of ordinary diarrhœa, although a more

typical case of typhoid fever, hitherto called infantile remittent fever, could not possibly fall under observation. This case was, I have no doubt, only one of numerous instances of infantile typhoid fever that every autumn pass for other wholly different affections.

Having determined with certainty that the fever prevailing in and around Melbourne during last autumn was genuine typhoid, it now becomes necessary to examine the replies to the second question—whether it was more prevalent, severe, or fatal than in former years.

Unfortunately, as with the first question, the replies were very conflicting, some averring that the fever was more, others that it was less severe or fatal. All may have been right, as far as individual experience went, but yet the aggregate of answers alone would afford a true idea of the extent or type of the fever. This is a good example of the utter uselessness of trusting to isolated observations in such a matter, and a refutation of those who object to the application of statistics to medical science; for here we must either take what is really worse than no estimate—a conflicting account—

or have recourse to public statistics. Fortunately, these are at hand, as admirably arranged as they are ample. A reference to Table I., showing the number of cases of typhoid fever treated in the Melbourne Hospital during the period from 1860 to the end of May 1874, with the number of deaths during that time, proves that last February was the most fatal month, there having been 7 deaths; this was followed by the next most fatal month, March, with 6 deaths; April, with a severe mortality; and after that the most fatal May, with 6 deaths. The years 1866 and 1870 were the next most severe years, while May, as a rule, was the worst month. Mention is here made only of cases set down as typhoid; thus, in 1866 there were in all 194 cases of fever treated in the hospital, of which 24 were fatal, yet only 70 of these, with 16 deaths, were ascribed to typhoid fever; and again, in 1870 there were 211 cases of fever, with 29 deaths, but only 97, with 19 deaths, were alleged to be typhoid. It would not be a great venture to hazard an opinion that all the cases were typhoid, a good reason being at hand to account for the apparent disparity;

but, unwilling to appear invidious even in conjecture, I would rather that some gentleman attached to the hospital gave the explanation.

Turning next to the total returns for the city and suburbs, the same relationship of fever to season as was observed in the tables for the hospital, is brought out, general and special returns closely agreeing. Thus, in Table III.,¹ January, February, March, and April last are shown to have had the largest absolute mortality from fever of any corresponding season for fourteen years, the years 1866 and 1870 being the next to the present year in fatality.

But a more accurate test than that of *absolute* mortality has been applied, in the computed percentages of deaths from typhoid fever to population, which bring, however, the same fact as is revealed by the gross returns of the whole

¹ In this table typhus and infantile remittent fevers are named, because they have hitherto in a loose way always been so mentioned in registration returns. Now, however, the Government statist has become more exact in the nomenclature, classifying all fevers as typhoid, unless otherwise specially registered by medical men. That is to say, all such terms as "gastric fever," "low fever," &c., he translates into typhoid fever.

TABLE III.
MELBOURNE AND SUBURBS.

POPULATION, AND DEATHS FROM TYPHUS AND INFANTILE FEVER, TYPHOID, &c. 1860-1874.

Year.	Estimated Population.	Number of Deaths from Typhus, Typhoid, &c.												
		Total in each Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1860	134,240	121	7	11	9	14	17	13	11	8	10	7	7	7
1861	139,916	82	12	16	8	11	10	7	11	6	2	4	2	3
1862	144,050	96	2	4	6	7	23	9	7	4	6	12	6	10
1863	148,873	129	11	9	11	18	19	16	10	16	6	9	1	3
1864	154,385	80	11	9	7	14	6	6	3	6	8	2	3	8
1865	160,586	107	8	8	12	12	16	7	10	2	3	6	0	14
1866	167,476	172	7	16	26	21	25	20	8	9	7	12	8	13
1867	174,366	123	9	13	16	15	17	9	10	7	6	6	7	8
1868	181,945	126	6	7	3	12	17	13	16	7	13	12	12	8
1869	190,212	116	7	7	11	13	13	10	11	3	8	9	13	11
1870	199,169	171	14	11	17	32	22	22	14	10	9	8	8	4
1871	208,814	126	15	14	17	12	20	10	9	7	5	7	3	7
1872	225,203	112	12	14	7	12	14	8	11	10	5	7	4	8
1873	233,047	115	10	10	12	16	13	12	10	5	4	8	7	...
1874	16	26	33	37	30

population. By this mode of reckoning, as is shown in Table IV., last February appears to have been, in proportion to population, the most fatal February in the past fourteen years, deaths having been 1.08 in every 10,000 of the population; the next highest being February of 1866, with 0.96 per 10,000, living. March last was 1.37, but was exceeded by March of 1866 with 1.55. Then April had 1.54, the highest except April of 1870, with 1.61. May last was also large, with 1.25, although that proportion for the month has been occasionally exceeded in former years. The year 1866 was the severest year of all, the ratio of deaths to population having been 10.29 for the year; but, of course, it is uncertain till the end of the present year how far 1874 may compare with those of the past. As far as the year has gone, the mortality indicates about the same proportion.

This is the conclusion to be drawn from the returns. It can only be disputed by disputing the accuracy of the registrations upon which the calculation is based; and if these be not accurate, the fault is entirely due to medical

TABLE IV.
MELBOURNE AND SUBURBS.

Population and Deaths from Typhus and Infantile Fever, Typhoid, &c., 1860-1873.
TABLE SHOWING THE DEATHS FROM TYPHUS AND INFANTILE FEVER, TYPHOID, &c., PER 10,000
OF THE POPULATION OF MELBOURNE AND SUBURBS.

Year.	Estimated Population of Melbourne and Suburbs.	Number of Deaths from Typhus, Typhoid, &c., per 10,000 of the Population.												Total in each Year.
		Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	
1860	134,240	.52	.82	.67	1.04	1.27	.97	.82	.60	.74	.52	.52	.52	9.01
1861	139,916	.86	.43	.57	.79	.71	.50	.79	.43	.14	.28	.14	.21	5.85
1862	144,050	.14	.28	.42	.49	1.60	.62	.49	.28	.42	.84	.42	.69	6.69
1863	148,873	.74	.60	.74	1.20	1.28	1.07	.67	1.07	.40	.60	.07	.20	8.64
1864	154,385	.72	.39	.45	.90	.39	.39	.20	.13	.52	.13	.20	.52	5.20
1865	160,586	.50	.50	.75	.75	1.00	.44	.62	.13	.19	.38	.57	.88	6.71
1866	167,476	.42	.96	1.55	1.26	1.49	1.19	.48	.54	.42	.72	.48	.78	10.29
1867	174,366	.52	.75	.92	.86	.97	.52	.57	.40	.34	.34	.40	.46	7.05
1868	181,945	.33	.39	.16	.66	.93	.71	.88	.39	.34	.66	.66	.44	6.92
1869	190,212	.37	.37	.58	.68	.68	.53	.58	.16	.42	.48	.68	.58	6.11
1870	199,109	.70	.55	.85	1.61	1.10	1.10	.70	.50	.45	.40	.40	.20	8.56
1871	208,814	.72	.67	.81	.57	.96	.48	.43	.33	.24	.33	.14	.33	6.01
1872	225,203	.53	.62	.31	.53	.62	.36	.49	.44	.22	.31	.18	.36	4.97
1873	233,047	.43	.43	.51	.69	.56	.51	.43	.22	.17	.34	.30	.34	4.93
1874	during the period	7.50	7.76	9.29	12.03	13.56	9.39	8.15	5.88	5.38	6.33	5.16	6.51	96.94
	*240,891	.66	1.08	1.37	1.54	1.25

* Assumed population.

men themselves, who inaccurately register the cause of death.

But mere mortality does not show the full extent of the fever, nor are there any means of ascertaining this extent except approximately by computation. There is no registry of cases of fever unless when they prove fatal, so that whether the fatal cases bear any definite relation to the actual number of cases of fever or not is unknown. All that can be said is that, since the fever obeys here the same general laws that it observes in other countries, the relative mortality is probably the same as it has been proved to be in those countries. Calculating by this very fair analogy—and many seem to be of opinion that the fever is not so deadly in this as in some other countries—an estimate may be made of the number of persons who have passed through the fever in a given year, and therefore of the extent of disabling and distress it has occasioned. The proportion of deaths to recoveries in England has been computed as one in nine. Dr. Murchison has found it as high as one in six. Dr. Budd and Mr. Simon reckoned one in nine-and-a-half as

the average mortality of the Windsor epidemic of 1858. But as that was an unusually severe outbreak, the mortality was higher than the average of the whole kingdom, which is generally estimated as about one in ten cases. At Sandhurst Hospital it was found to be from 15 to 18 per cent.¹ Taking the rate of mortality in this city at the lower estimate, it will be found that as there were in the first five months of this year 142 deaths from fever, these being multiplied by 10 give 1,420 cases of fever during that period in Melbourne and suburbs. There is no need of suggesting to the imagination what suffering and misery this amount of fever implies. "It is a fundamental axiom in scientific investigation that our emotions should be rigidly excluded."²

As it will certainly become a question with some inquirers whether this fever rate was relatively high or low, it is expedient to anticipate the question by a distinct answer; and, taking the fever death-rate of England and Wales as a standard of comparison, the following tabulated

¹ *Aust. Med. Jour.*, Aug. 1867.

² *Typhoid Fever*, p. 3.

observations of the Government statist are ready to hand for the purpose. From these it appears that in Victoria in ten years there have been 3,469 deaths from typhoid fever, representing 34,690 cases of a fever which with care equal to that taken for prevention of small-pox would become as great "a pathological rarity." Thus :—

"The proportion that deaths from each class of diseases bear to the total deaths in Victoria and England and Wales is shown in the following table :—

DEATHS FROM EACH CLASS OF DISEASES, PROPORTION TO DEATHS FROM ALL CAUSES, IN VICTORIA AND ENGLAND AND WALES.

Causes of Death.	Number of Deaths in 1,000 from all Causes.		
	Victoria.		England and Wales.
	Year 1873.	Average of 16 Years.	Average of 10 Years.
Zymotic diseases...	254·51	304·71	227·43
Constitutional „ ...	139·56	130·56	187·54
Local „ ...	386·39	320·05	388·81
Developmental „ ...	140·69	151·97	161·17
Violence ...	78·85	92·71	35·05
Total ...	1,000·00	1,000·00	1,000·00

"Relatively to the total mortality, that from zymotic diseases, including all those of an endemic, epidemic, contagious or infectious character, is greater in Victoria than in England and Wales.

“A better way, however, of ascertaining the relative mortality of the two countries is to compare the deaths from each class of diseases with the total population of each country, and this is done in the following table :—

DEATHS FROM EACH CLASS OF DISEASES, PROPORTION
TO LIVING POPULATION, IN VICTORIA AND ENGLAND
AND WALES.

Causes of Death.	Number of Deaths to every 1,000 of Mean Living Population.		
	Victoria.		England and Wales.
	Year 1873.	Average of 16 Years.	Average of 10 Years.
All causes	14·74	17·21	22·47
Specified causes ...	14·69	17·04	22·32
Zymotic diseases...	3·74	5·28	5·07
Constitutional ,, ...	2·05	2·21	4·19
Local ,, ...	5·67	5·40	8·68
Developmental,, ...	2·07	2·58	3·60
Violence	1·16	1·57	·78

“It will be observed that, in proportion to the population, a lower rate of mortality from all classes of diseases prevailed in Victoria during the year under review than in England; but that, on the average, the mortality from zymotic diseases is somewhat higher.

“Zymotic diseases are generally most prevalent in the summer months, and are especially fatal to children. A complaint which forms an exception to this rule is typhoid fever, from which adults suffer also, and which is most common in the months of April and May, although sporadic cases occur all the year round. As considerable

alarm existed a short time since in respect to this complaint, I have thought it sufficiently important to devote to it a separate table, showing the number of deaths and their proportion to the living population during each of the last ten years :—

DEATHS FROM TYPHOID FEVER, 1864-1873.

Year.	Estimated Mean Popula- tion.	Deaths from Typhoid Fever.	
		Total Number.	Number per 10,000 of Population.
1864	589,160	250	4.24
1865	616,375	291	4.72
1866	634,077	528	8.33
1867	649,826	455	7.00
1868	671,222	295	4.40
1869	696,942	360	5.16
1870	709,839	416	5.86
1871	738,725	269	3.64
1872	760,991	323	4.24
1873	780,362	282	3.61
Mean of Ten Years	684,752	347	5.07

“ It will be noticed that the death rate from this disease differs considerably in different years, it having been lowest in the year under review (1873), and highest in the years 1866 and 1867. In the two last-mentioned years the mortality from all causes was also above the average. It is well-known that a wide-spread belief gained ground during a portion of the present year (1874) that this complaint was prevailing in the colony to an unusual extent, and in a very fatal form. Whether this supposition was

correct, and, if so, to what extent the total mortality of the year will be affected by the disease, cannot be known until after the year has been completed."

It will be seen by referring to Table III. that the first five months of 1874 have been, as regards typhoid fever, among the most fatal recorded here; but whether the mortality exceeded the average of typhoid fever in England, has not yet been shown.

The next question, relating to conditions common among the affected localities as suggesting some general cause of fever, has had but slight elucidation from the replies. Bad drainage, decomposing organic matters, imperfect ventilation, &c., are all ready phrases, implying a foregone opinion of the cause of typhoid fever; and from the fact that to such conditions the fever is by the majority of medical men ascribed is to be inferred their belief in the spontaneous origin of fever miasm; and further, from the absence of the term contagion from most of the replies, that the greater number of medical men here are non-contagionists. That foul drains, putridity, and impure air are common conditions of our city

life is only too evident to the senses ; but that this "*civic miasm*" is the generator of the specific poison of typhoid fever has never yet been thoroughly demonstrated to the reason.

This question of localisation of fever might be elaborately discussed in detail at great length without arriving any nearer to a solution of the question of causation than is comprehended in the general conclusion here above expressed. That the fever has not been localised is abundantly evident from the information contained in the answers of correspondents, who have written from personal observations, information which my own personal inspections have corroborated. Fever has been prevalent alike on the hilly sites of Kew and Toorak, and on the flats of Prahran, Williamstown, and Collingwood ; the clay soil and subsoil of Williamstown have not been more fertile of fever than the light sandy soil and gravelly subsoil of Prahran, nor have the rough drains of *cul-de-sac* alleys been more conducive than the highly-pitched channels of through streets. One gentleman carefully observed that "in Richmond the fever (typhoid, as diagnosed by him) has been equally severe and fatal on

the higher part, where the houses are large, and well ventilated, and the drainage good; as on the flats where drainage is imperfect, and manufactories, such as tanning, wool-washing, &c., &c., are carried on." Another observer remarked of Kew and Hawthorn, that "the deaths have occurred in all parts of the district; in one-half, the houses were in healthy elevated localities; the others were in low-lying districts, and where domestic and local peculiarities may have given some reason to think of a special cause. Our water supply is good; drainage is by no means bad; and everything that can be done is effected by the local board of health. In one house three or four suffered, of whom two died. Judging from what I have seen during the last few months, I have little doubt of the infectious nature of the disease. In one case a patient left for her home (six miles away) after being ill a fortnight; she died. Her brother was ill for weeks, although not exposed to the same causes as she had been; and this is not the only case I have seen of the same kind. In my opinion want of ventilation and cleanli-

ness, and over-crowding is more conducive to diseases of this type than want of out-door supervision." A third correspondent more briefly summarises the results of his observations of external conditions in these few words:— " They (the fever cases) sprung up all over the place, without regard to rich or poor, or high situations or the reverse." A young man, who died, fancied he caught the fever while working amongst the river mud in a dredging machine. One practitioner resident in Williamstown had generally inferred that the clay soil there was a special local cause of typhoid, but he felt a difficulty in reconciling that view with the fact of the same fever occurring on the sandy soil on the opposite side of the bay. Another physician of the same town laid more stress on the habitual over-crowding of small sleeping rooms in narrow ill-drained lanes ; but yet he saw the reasonable objection that such causes were held more likely to generate *typhus* than typhoid fever. Some other practitioners would attach importance to the forced artificial ground on which the foundations of some new houses are built ; although

not a single instance of typhoid fever occurring under such circumstances has been brought under my notice.

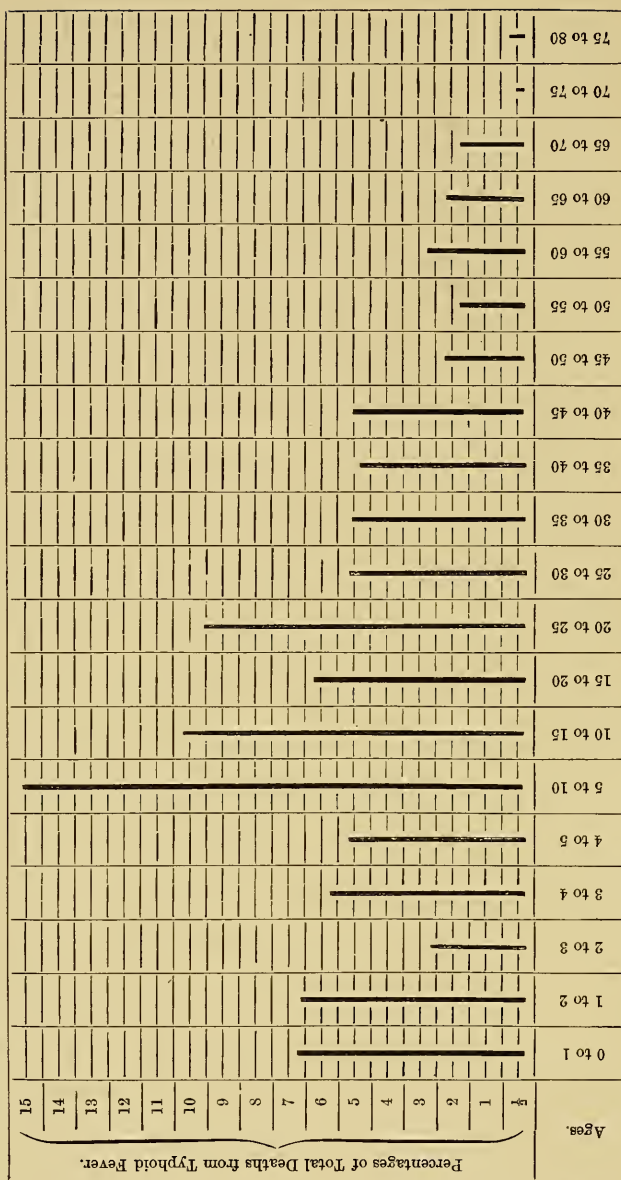
In some localities it has thus been seen that the miasm is always present, or at least its sources are; not so the fever, which observes strict periodicity in its visits. This suggests some factors besides putrescence for its production. The action of these will be better discussed together under the heading of the seventh question, on seasonal peculiarity, and till then I shall defer its consideration.

In next taking up the answers to the fifth question, as to peculiarity among the persons attacked making them prone to fever, it will be found that the statements on this point may be brief, since the statistics make the answer obvious. From these it will be found that youth has chiefly determined personal liability. From Table V., giving the ages of those who died of typhoid fever during the five years 1869-73 inclusive, it appears that, of a total of 640 deaths, 468 were under 30 years of age, and only 172 over 30 years. Accompanying the table is a diagram showing at a glance

TABLE V.
MELBOURNE AND SUBURBS.
DEATHS OF PERSONS OF VARIOUS AGES FROM TYPHOID FEVER IN THE FIVE YEARS 1869 TO 1873.

Year.	Under 1 Year.	1 to 2 Years.	2 to 3 Years.	3 to 4 Years.	4 to 5 Years.	5 to 10 Years.	10 to 15 Years.	15 to 20 Years.	20 to 25 Years.	25 to 30 Years.	30 to 35 Years.	35 to 40 Years.	40 to 45 Years.	45 to 50 Years.	50 to 55 Years.	55 to 60 Years.	60 to 65 Years.	65 to 70 Years.	70 to 75 Years.	75 to 80 Years.	Total.
1869	8	12	1	5	6	19	9	5	14	8	6	5	7	1	1	4	4	1	116
1870	18	7	5	7	11	27	18	11	20	12	9	8	9	1	2	2	2	171
1871	11	13	1	7	8	19	6	10	10	3	6	9	7	2	3	7	1	1	126
1872	4	8	5	7	5	13	16	9	11	3	6	7	2	4	3	2	3	112
1873	3	3	4	10	3	18	17	4	6	8	6	3	8	7	1	2	3	4	3	2	115
Total	44	43	16	36	33	96	66	39	61	34	33	32	33	15	10	17	13	12	3	4	640
Percentage	6·87	6·72	2·50	5·62	5·16	15·00	10·31	6·09	9·53	5·31	5·16	5·00	5·16	2·34	1·56	2·65	2·03	1·88	·47	·63	100·00

DIAGRAM ILLUSTRATING THE PERCENTAGE OF DEATHS AT VARIOUS AGES FROM TYPHOID FEVER, IN MELBOURNE AND SUBURBS, DURING THE FIVE YEARS 1869 TO 1873, AS SHOWN IN TABLE V.



the ages at which the fever is most liable to set in, from 5 to 10 years, and from 10 to 15, being the two worst lustra ; and from 20 to 25 the next.

Those ages correspond exactly with the ages most liable in England and elsewhere, the fever being usually one of infancy and adolescence rather than of middle age. This table would have been more complete if it had given the number of persons living at the various ages ; still, as it is, the inference as to the predisposing tendency of youth to typhoid fever is, I feel certain, quite accurate. Here, again, the laws of the fever agree with its laws observed elsewhere, giving incidentally fresh proof that our fever is in reality a genuine typhoid fever. There is no other circumstance common to the persons attacked rendering them liable, because it is universally admitted that the fever indiscriminately attacks persons in all grades and conditions of social life ; it seizes upon the rich and well-housed as readily as upon the poor and squalid ; it even attacks the strong and healthy rather than the weak. “ By reason of our common humanity, we are all, whether rich

or poor, more nearly related here than we are apt to think.”¹

Passing on to the seventh question—Have seasonal peculiarities influenced the fever?—many points of great interest become apparent on examining the accompanying tables. The influence of season has always been recognised here in connexion with the prevalence of fever. The sun shines on rich and poor alike; and rich and poor alike supply the conditions out of which the sunshine either concocts, according to one theory, or desiccates and spreads about, according to another, the infecting or the contagious agent of the deadly fever.

Medical men are almost unanimous in attributing great influence to the seasons, and the returns confirm the opinion. From Table IV., page 42, it will be seen that although the fever is never wholly absent during any month of the year, yet that by far the larger proportion of cases occur in autumn. May is the severest month; November the least so. In the aggregate of cases for fourteen years the mortality in

¹ *Typhoid Fever*, p. 183.

May averaged 13·56 in every 10,000 persons living, while in the corresponding November month it reached only 5·16. This, taking the months, is exactly the converse of what takes place in England, where the least severe month is April, and the worst month is October. This is fully shown in a table and diagram at p. 446 of Dr. Murchison's work on fever. The number of admissions to the Melbourne Hospital and to the London Fever Hospital entirely agree in illustrating this seasonal influence. Thus, in twenty-three years, there were 209 admissions to the latter in April and 839 in October ; while, in eleven years, 134 were admitted into the Melbourne Hospital in May and only 12 in October ; or, by the months, just the reverse, while agreeing in seasons at the Northern and Southern Hemispheres.

These details supply no new fact, it is true, and only amplify what is already known as a general principle ; but if they appear superfluously minute, it should be borne in mind that they are all-important in studying the question of etiology. Indeed, through such data alone will the cause be finally traced.

In connexion with those details, there is one other fact of much significance in causation, that, as far as I can learn, has not received much attention here. It is the most invariable precedence of diarrhœa during summer to typhoid fever in autumn. This diarrhœa usually begins in November, increases in December, and culminates in January, and with its decline typhoid begins, increasing month by month, to culminate in May; years that are bad for the one disease being usually bad for the other. The same order of succession has been observed in England. Dr. Murchison remarks:—"It is also interesting to note that the ordinary autumnal increase, or circumscribed epidemics, of enteric fever, are usually preceded by a great prevalence of diarrhœa, the diarrhœa reaching its acme long before the fever does, and having greatly declined by the time that the latter is most prevalent." Whether diarrhœa has, or has not, genetic relations to fever, of which it is the constant precursor, remains to be discovered; but the following tables enable a test comparison to be made with similar data in other countries.

TABLE VII.
MELBOURNE HOSPITAL.
RETURN OF CASES ADMITTED AND OF DEATHS FROM DIARRHŒA FROM JANUARY 1860
TO MAY 1874.

Year.	January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Totals.	
	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.	Total treated.	Died.
1860	4	—	5	2	2	1	1	1	1	—	1	—	2	—	—	—	3	—	1	—	—	—	—	—	20	4
1861	1	—	—	—	3	1	1	1	—	—	2	—	1	—	—	—	—	—	—	—	1	—	1	—	14	3
1862	—	—	2	—	2	—	3	1	—	—	1	—	1	—	1	—	—	—	1	—	1	—	1	—	15	2
1863	2	—	2	—	1	—	1	—	2	—	3	1	1	—	3	1	4	2	2	1	—	—	—	—	28	4
1864	10	2	5	—	1	—	4	—	1	—	3	—	1	—	1	—	1	—	2	—	2	—	—	—	30	3
1865	3	—	1	—	2	—	4	—	1	—	1	—	1	—	1	—	1	—	—	—	—	—	—	—	25	3
1866	7	2	3	1	4	2	4	2	2	—	1	1	2	—	3	—	2	—	3	1	3	1	9	6	43	11
1867	3	1	3	—	5	1	3	—	2	—	1	—	2	—	—	—	3	—	1	—	1	—	—	—	23	6
1868	8	1	3	—	3	2	2	—	3	—	2	—	1	—	2	—	3	—	2	—	3	—	9	2	44	4
1869	5	1	2	1	1	1	2	—	4	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	25	5
1870	5	—	2	1	3	2	3	1	—	—	—	—	—	—	1	—	2	—	1	—	2	—	6	1	24	5
1871	6	—	6	2	2	—	1	—	3	—	2	—	2	—	1	—	2	—	3	—	4	—	5	1	37	4
1872	11	2	4	2	3	—	3	—	—	—	1	—	1	—	2	—	3	—	2	—	4	—	7	1	43	4
1873	9	2	4	2	3	1	3	1	3	—	1	—	—	—	1	—	1	—	1	—	2	—	2	—	35	9
1874	2	2	5	2	4	1	4	1	3	1	—	—	—	—	—	—	—	—	—	—	2	—	—	—	24	7
	81	13	47	12	38	11	38	8	33	1	20	2	15	—	17	1	23	2	21	2	26	5	71	17	430	74

TABLE VIII.
MELBOURNE AND SUBURBS.
POPULATION, AND DEATHS FROM DIARRHOEA, 1860-1874.

Year.	Estimated Population.	Number of Deaths from Diarrhoea.												
		Total in each Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1860	134,240	403	104	26	140	22	5	3	3	14	5	8	24	49
1861	139,916	226	65	37	24	26	9	6	6	1	3	4	13	32
1862	144,050	351	64	57	56	20	12	6	7	3	5	5	39	77
1863	148,873	249	90	31	31	23	15	5	8	2	2	4	4	34
1864	154,385	287	68	63	41	36	8	3	1	3	1	1	10	52
1865	160,586	448	91	73	60	46	30	17	8	4	5	9	27	78
1866	167,476	481	119	70	71	49	33	17	4	7	2	7	25	77
1867	174,366	446	131	99	64	38	12	9	10	2	9	7	18	47
1868	181,945	324	45	47	74	28	23	10	6	2	4	4	25	60
1869	190,212	363	90	67	39	38	15	8	5	4	4	10	26	57
1870	199,169	313	56	47	52	47	13	3	6	6	3	...	26	54
1871	208,814	278	67	56	34	23	13	5	2	2	2	4	15	55
1872	225,213	335	68	65	50	24	10	10	2	7	7	9	35	48
1873	233,040	293	40	39	42	29	15	4	11	3	7	3	33	67
1874	91	86	56	22	15

TABLE IX.
MELBOURNE AND SUBURBS.

Population and Deaths from Diarrhœa, 1860-1873.

TABLE SHOWING THE DEATHS FROM DIARRHŒA PER 10,000 OF THE POPULATION OF
MELBOURNE AND SUBURBS.

Year.	Estimated Population.	Number of Deaths from Diarrhœa per 10,000 of the Population.												Total in each Year.
		Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	
1860	134,240	7.75	1.94	10.43	1.64	.37	.22	.22	1.04	.37	.06	1.79	3.65	30.02
1861	139,916	4.65	2.64	1.71	1.86	.04	.43	.43	.07	.21	.29	.93	2.29	16.15
1862	144,050	4.44	3.96	3.89	1.39	.83	.42	.49	.21	.35	.35	2.71	5.35	24.39
1863	148,873	6.05	2.08	2.08	1.54	1.01	.34	.54	.13	.13	.27	.27	2.28	16.72
1864	154,385	4.40	4.08	2.66	2.33	.52	.19	.06	.19	.06	.06	.65	3.37	18.57
1865	160,586	5.67	4.55	3.74	2.86	1.87	1.06	.50	.25	.31	.56	1.68	4.86	27.91
1866	167,476	7.11	4.18	4.24	2.93	1.97	1.02	.24	.42	.12	.42	1.49	4.60	28.74
1867	174,366	7.51	5.68	3.67	2.18	.69	.52	.57	.11	.52	.40	1.04	2.70	25.59
1868	181,945	2.47	2.58	4.07	1.54	1.26	.55	.33	.2122	1.37	3.30	17.80
1869	190,212	4.73	3.52	2.05	2.00	.79	.42	.27	.21	.21	.53	1.37	3.00	19.10
1870	199,169	2.81	2.36	2.61	2.36	.65	.15	.30	.30	.15	...	1.30	2.71	15.70
1871	208,814	3.21	2.68	1.63	1.10	.62	.24	.10	.10	.10	.20	.72	2.63	13.33
1872	225,213	3.02	2.89	2.22	1.07	.44	.44	.09	.31	.31	.40	1.55	2.13	14.87
1873	233,040	1.72	1.67	1.08	1.24	.64	.17	.47	.13	.30	.13	1.42	2.88	12.57
during the period * 240,891		65.54	44.81	46.80	26.04	12.30	6.17	4.61	3.58	3.14	4.43	18.29	45.75	281.46
1874		3.78	3.57	2.32	0.91

* Assumed population.

The same order having been constantly noticed here, it can hardly be doubted that the diseases are of like character, and arise from like causes; and that whatever applies to them in the one country will be found equally to hold good in the other. This feature could be best exhibited at a glance on charts, where, in almost every year, the lines traced would indicate fever and diarrhœa to begin in January, and show two generally diverging tracks, one of fever rising as the other of diarrhœa falls.

On comparing such lines with those on other charts of the changes of the barometer, temperature of the air, ozone, humidity, and rainfall, the lines of the barometer, ozonometer, and humidity would be seen to rise as the line of temperature fell, the line for fever rising with the former, and that for diarrhœa falling with the latter, as if a constant natural law involved both phenomena. A contrast of the years 1866 and 1873 will exemplify this. In 1866 diarrhœa was 28·74 as compared with 12·57 in 1873, or more than twice as fatal. In January of 1866 it was 7·11 to 1·72 in January of 1873, or six times as fatal. In 1866 fever was 10·29 to 4·93

in 1873. In May of 1866 it was 1·49 to 0·56 in May 1873. In 1866 the air was hot and dry ; in 1873, cool and moist. The relation of this state of atmosphere to fever was noticed at Sandhurst :—“ In the year 1866, in the early part of which the district was suffering from great heat and an almost unprecedented drought, the number of admissions for fever was much larger than in any of the other years.”

This relation of atmospheric states to diarrhœa and typhoid fever may be more clearly illustrated in a tabular form, as follows :—

CONTRAST OF THE YEARS 1866 AND 1873, IN DRYNESS AND MOISTNESS OF AIR, WITH INCREASE AND DIMINUTION OF DIARRHŒA AND TYPHOID FEVER.

Years.	Diarrhœa.		Fever.		States of Air.
	For the Year.	For January.	For the Year.	For May.	
1866	28·74	7·11	10·29	1·49	Hot and dry.
1873	12·57	1·72	4·93	0·56	Cool and moist.

Season, therefore, greatly influences typhoid fever, and certainly in a way quite as consistent with the facts of contagion as with those that seem to support the pythogenic hypothesis.

On this hypothesis it is held that the exciting cause of the fever, an unknown something concocted in putrescing organic, especially fæcal, stuff, and diffused through air, or food, or drinking water, is merely quickened into greater action during the protracted heat of summer and autumn, while it becomes less active or inert during the protracted cold of winter and spring. On the other hand, the theory of contagion affirms that a specific typhous virus, shed from diseased glands in the bowels of persons suffering from fever, the special seat of the disease, on being exposed to dry hot air becomes crisped into a fine impalpable dust, easily borne on the air, and when inhaled, or mixing with drinking water or food, and therewith ingested, excites fever; and further, that this imponderable poison-dust is readily washed out of the atmosphere by the heavy rains of winter and spring.

The following Tables show the monthly and yearly states of barometer, temperature of air, ozone, humidity, rainfall, and number of days of rain, from which the illustrative charts referred to at page 62 could be constructed.

Months.	1860.						1861.						1862.					
	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days of Rain.	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days of Rain.	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days of Rain.
January ...	Inchs. 29.74	68.5	7.1	63	1.97	10	Inchs. 29.83	67.4	6.8	67	2.25	14	Inchs. 29.82	68.1	...	59	1.25	4
February ...	29.93	65.6	7.0	64	1.08	4	29.79	65.0	9.2	74	4.62	13	29.85	66.5	...	63	0.19	8
March ...	29.91	65.7	7.8	67	0.96	6	29.96	65.5	6.4	66	2.65	9	29.92	65.6	8.3	67	1.08	4
April ...	29.95	58.8	9.6	77	4.53	12	30.01	59.3	8.8	76	1.29	11	29.98	57.2	10.3	73	3.56	16
May ...	30.01	54.8	9.2	73	0.99	11	29.98	52.8	8.4	76	0.84	12	29.92	52.7	10.1	78	4.31	10
June...	29.95	49.7	10.6	79	1.72	16	29.83	50.4	...	77	1.78	16	30.01	48.5	11.0	82	2.99	14
July ...	30.19	46.7	11.8	82	1.21	9	29.96	46.1	...	84	2.14	16	29.83	50.5	12.8	80	2.26	24
August ...	30.17	50.1	11.0	79	0.79	10	30.08	49.5	...	79	1.47	14	30.06	50.8	11.8	75	1.95	11
September ...	29.93	53.8	9.8	70	2.72	17	29.94	54.4	...	70	3.19	17	29.82	54.2	...	72	0.98	13
October ...	29.96	57.0	10.6	71	1.97	15	29.84	58.7	...	66	4.89	14	30.02	58.0	10.6	73	2.03	19
November ...	29.83	60.6	7.3	67	2.38	12	29.93	61.0	...	64	1.46	11	29.86	64.1	9.0	59	0.32	9
December ...	29.84	64.7	7.4	67	5.06	11	29.75	62.5	...	63	2.58	12	29.81	63.9	8.5	63	1.16	7
Year ...	29.95	57.9	9.1	72	25.38	133	29.91	57.6	...	72	29.16	159	29.91	58.2	...	71	22.08	139

ON TYPHOID FEVER

Months.	1863.						1864.						1865.					
	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days
	Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.	
January ...	29.82	68.1	8.8	.63	1.84	9	29.83	63.9	5.4	.65	2.07	7	29.89	63.1	8.0	.63	0.16	3
February ...	29.85	66.3	9.6	.68	2.74	12	29.89	63.2	7.0	.67	2.63	9	29.86	64.6	7.6	.58	0.59	8
March ...	29.92	65.4	7.0	.69	3.84	14	30.07	63.4	7.0	.70	1.80	7	29.92	62.3	8.8	.66	1.26	8
April ...	30.10	58.6	7.0	.74	1.76	10	29.97	60.1	9.8	.77	4.53	18	30.00	60.9	7.2	.63	0.72	7
May ...	29.95	53.9	9.0	.79	2.54	16	30.13	53.7	9.8	.78	1.03	9	29.95	50.9	12.2	.76	3.41	18
June... ..	30.06	49.9	6.4	.85	1.16	10	30.05	49.1	10.2	.81	0.81	10	30.11	48.2	10.6	.78	1.64	10
July... ..	29.87	49.5	11.8	.77	2.87	16	29.90	48.1	11.2	.81	2.83	13	30.01	46.4	12.4	.79	2.05	14
August ...	29.97	48.8	13.0	.79	2.10	18	29.92	49.2	11.8	.76	2.50	18	30.05	49.2	10.8	.72	1.22	14
September ...	29.98	52.1	10.8	.71	1.99	15	29.91	54.9	10.0	.72	2.28	14	29.83	52.9	13.2	.68	1.87	13
October ...	29.65	56.1	10.2	.71	4.89	18	29.90	55.7	12.0	.73	4.08	18	29.96	56.1	11.4	.66	0.78	8
November ...	29.78	59.3	8.6	.66	3.51	13	29.91	61.0	8.0	.62	0.64	9	29.88	62.3	9.2	.61	0.89	7
December ...	29.78	62.7	6.8	.72	7.18	14	29.85	62.2	8.4	.65	2.21	12	29.76	61.0	11.2	.63	1.35	9
Year ...	29.89	57.5	9.1	.73	36.42	165	29.94	57.1	9.2	.72	27.40	144	29.94	56.5	10.2	.68	15.94	119

Months.	1866.						1867.						1868.					
	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days
January	Inchs. 29.85	° 66.3	0.21 9.8	Sat. 63	Inchs. 1.43	4	Inchs. 29.89	° 67.0	0.21 8.8	Sat. 63	Inchs. 1.92	6	Inchs. 29.84	° 63.8	0.21 10.6	Sat. 70	Inchs. 2.11	10
February	29.90	67.8	8.8	61	0.35	5	29.85	65.8	10.8	67	2.68	7	29.94	64.7	10.4	67	0.99	6
March	30.04	63.4	11.2	64	2.14	9	30.02	62.6	11.0	64	0.75	8	29.96	65.0	10.6	61	1.02	5
April	30.07	60.8	10.6	70	0.57	5	30.01	58.7	11.2	78	2.42	11	30.10	57.6	10.6	66	1.44	10
May	29.98	56.4	10.2	71	2.92	7	30.11	53.3	11.0	86	2.38	12	30.26	52.7	10.2	75	1.48	8
June...	30.12	49.3	9.8	80	1.64	11	30.03	52.2	11.2	79	1.08	7	29.93	49.2	11.4	79	1.21	11
July	30.00	47.5	12.2	82	2.04	11	29.92	49.3	13.4	78	1.66	20	30.07	47.4	13.8	79	1.46	15
August	30.04	50.3	12.0	75	2.26	15	30.10	51.0	12.4	79	1.91	14	29.99	48.6	11.8	76	1.01	10
September	29.87	52.4	13.8	70	2.08	14	29.74	53.1	13.6	74	3.43	18	29.94	54.8	12.4	71	2.13	15
October	29.80	58.3	12.6	68	3.25	15	29.67	56.8	12.2	68	3.35	18	29.92	57.6	12.0	65	1.05	11
November	29.88	58.0	13.2	67	2.17	10	29.91	60.1	11.0	63	0.87	3	29.97	61.5	11.6	69	3.19	11
December	29.90	63.3	8.0	65	1.56	4	29.75	62.0	10.8	63	3.34	9	29.83	62.7	10.6	63	1.18	8
Year	29.95	57.8	11.0	70	22.41	107	29.92	57.7	11.4	72	25.79	133	29.98	57.1	11.3	70	18.27	120

Months.	1869.						1870.						1871.					
	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days
	Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.	
January	29.75	65.7	10.6	.60	1.46	6	29.80	67.3	7.8	.60	3.15	4	29.84	67.5	10.3	.68	3.00	9
February	29.88	65.5	9.4	.62	0.89	10	29.91	66.5	8.3	.62	0.03	1	29.80	66.7	9.2	.73	3.23	6
March	29.99	62.7	8.8	.68	1.53	3	30.05	64.6	8.2	.67	0.34	3	29.99	60.2	10.3	.70	2.37	9
April	30.08	57.7	8.9	.73	1.27	7	29.97	60.8	8.8	.78	4.88	15	30.06	58.0	9.2	.72	1.29	6
May	29.95	51.7	9.7	.82	1.99	14	30.04	51.3	9.5	.81	2.78	10	30.00	54.6	9.3	.86	1.39	11
June	30.08	49.9	10.2	.81	2.37	13	29.92	50.3	12.6	.83	3.32	21	29.96	50.481	1.25	9
July	30.18	47.1	9.2	.81	1.13	13	30.05	46.6	10.6	.82	3.16	14	29.89	48.678	2.08	15
August	30.05	51.4	9.2	.68	1.65	10	29.79	48.9	12.0	.78	2.14	15	29.93	51.872	3.58	11
September	30.08	52.7	9.7	.71	1.58	13	29.92	51.5	13.2	.78	5.87	17	29.96	53.374	1.86	15
October	29.81	55.3	10.5	.76	7.61	20	29.95	58.0	12.0	.76	4.38	12	29.97	54.5	12.0	.73	2.88	11
November	29.78	60.9	8.0	.66	2.12	13	29.87	59.3	11.6	.73	3.23	13	29.85	60.4	11.1	.72	4.19	16
December	29.74	65.6	7.4	.59	0.98	7	29.88	63.6	10.5	.66	0.49	4	29.84	66.2	10.9	.69	3.05	7
Year	29.95	57.2	9.3	.71	24.58	129	29.93	57.4	10.4	.74	33.77	129	29.92	57.774	30.17	125

Months.	1872.						1873.						1874.					
	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days	Barometer.	Temperature of Air.	Ozone.	Humidity.	Rainfall.	No. of Days
	Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.		Inchs.	°	0-21	Sat. = 100	Inchs.	
January ..	29.81	69.1	10.0	.72	4.98	12	29.94	65.3	9.4	.69	2.07	7	29.88	65.7	11.7	.67	2.14	6
February ..	29.96	64.9	9.4	.70	0.99	5	29.88	66.1	11.1	.75	4.67	13	29.91	64.6	9.9	.61	0.88	5
March ..	29.92	65.3	8.5	.70	1.82	7	29.98	62.0	11.9	.71	1.84	8	29.96	62.4	11.8	.75	6.36	19
April ..	30.02	55.9	9.6	.77	1.50	13	29.96	59.7	11.9	.76	2.07	13	30.05	58.9	9.2	.78	1.71	6
May ..	30.00	52.0	8.3	.78	2.43	14	29.98	55.0	11.4	.73	1.31	13	29.96	52.4	11.9	.81	2.07	15
June... ..	29.81	51.6	10.5	.81	3.66	17	30.04	52.2	11.0	.84	2.69	17						
July	29.90	48.7	9.9	.81	2.17	13	30.11	46.4	12.4	.83	1.40	12						
August ...	30.02	47.1	11.6	.79	2.83	13	29.95	51.7	13.7	.75	2.07	14						
September ..	30.06	53.8	10.1	.70	1.31	8	29.86	53.3	13.9	.71	2.57	12						
October ...	29.92	57.0	11.0	.70	2.86	11	29.89	59.3	12.5	.65	2.92	11						
November ...	29.87	62.8	10.4	.75	4.50	14	29.92	57.7	12.9	.70	1.41	10						
December ...	29.78	62.7	8.2	.68	3.47	9	29.82	67.2	9.1	.55	0.59	4						
Year ..	29.92	57.6	9.8	.74	32.52	136	29.94	58.0	11.7	.72	25.61	134						

Coming then to consider the answers to the sixth question—Were there any proofs of contagion?—the subject of origin or spread will be investigated. The current opinion, as deducible from the answers, are that typhoid is not contagious, and these agree with the general belief of the vast majority of the profession in Great Britain. Only one replied emphatically—“Yes; most distinct.” Another replied—“Many proofs; yet many cases seemed to arise *de novo*.” One gentleman was — “doubtful on the point.” Another said—“This is a troublesome question;” yet his own opinion, “though it is greatly disputed, is that it is contagious, *or rather infectious*;” while a fuller reply is that—“The proof is, that more than one in the same house became affected.”

The general opinion of medical men being thus decidedly against the doctrine of contagion, it is clear that their practice will not be marked by very strenuous efforts to prevent what they do not believe exists; so that, if typhoid really be after all a contagious fever, there is never much hindrance put to its spreading in this manner, and it becomes a vitally important

question to determine whether the general belief be right or wrong, well or ill-founded. The usually accepted pythogenic theory no doubt has led many to discard all notion of contagion, and to adhere to the theory of spontaneous origin in almost every case. But this view of its origin has recently sustained some severe shocks, and requires a thorough reconsideration. Professor William Stokes says—"The opinion was almost universally held that this form of fever was non-contagious, but the adherents to this doctrine are becoming less numerous of recent years."¹ And Professor Trousseau states, that "The number of the disbelievers in contagion is daily diminishing. The contagious character of typhoid fever is among the ascertained facts of science."² This author's admission that, "as it is obvious that typhoid fever at some time or another had a beginning, we cannot refuse to admit the possibility of its arising spontaneously," has been held by Dr. Murchison as in concurrence with his own views. But it is clear that many things that once had a begin-

¹ *Lectures on Fever*, p. 80, 1874.

² *Clinical Lect.*, Syd. Soc. Trans., vol. ii. p. 370.

ning now no longer begin *de novo*, as has been justly remarked by Niemeyer.¹ Sir Thomas Watson observes that "If once a disorder of this kind is clearly proved to be sometimes the effect of contagion (and this I think I shall be able to prove to you of continued fevers), we cannot help entertaining a doubt whether the disorder in question really has ever in reality any other cause."² As in small-pox, so in typhoid, contagion is, according to Dr. William Budd, "the master-fact in its history."³ On the other hand, Dr. Murchison, admitting the contagiousness of small-pox, declares that we "cannot reason from one specific fever to another, as the law of every one is special and peculiar."⁴

In his celebrated paper on the non-identity of typhus and typhoid fevers, Sir William Jenner incidentally adduces the superior degree of contagiousness of typhus as one mark of difference in character and causation. Yet, he alludes to

¹ *Pract. Med.*, vol. ii. p. 524.

² *Lectures*, vol. ii. p. 847, ed. 1871.

³ *Typhoid Fever*, p. 38, 1873.

⁴ *Continued Fevers*, p. 97, 1873.

both fevers as if the specific cause of each were "an influence emanating from the bodies of those affected." This hesitancy proves that the question of contagion was, even to him, involved in doubt.¹

The long intervals between outbreaks of typhoid fever, held by Dr. Murchison to prove spontaneous origin, seem, however, quite as compatible with carried contagion.²

Again, in drawing an analogy between puerperal fever, which "we have almost the power of generating at will," and typhoid, to prove the probable spontaneous origin of the latter, Dr. Murchison almost appears to ignore his own canon not to argue from one fever to another, whose laws widely vary. In this city we have had ample proof of the origin of puerperal fever in a Lying-in hospital, and of its spreading from the focus over the country. The hospital mortality has year by year increased from 1 death in 100 deliveries, to 1 in 74, 1 in 64, and finally last year, to 1 in 27. This truly "enormous" mortality has been followed by the

¹ *Med. Chir. Trans.*, vol. xxxiii. p. 41.

² *Med. Chir. Trans.*, vol. xli. p. 255.

almost equally unparalleled mortality of 1 death in every 31 deliveries in Melbourne, and of 1 death in every 164 deliveries amongst the whole population. Such wholesale slaughter demands the instant closure of the pest-house, fed and fostered by matrons wholly oblivious of the cruelty of alluring poor women into a false sanctuary, as well as of the glaring truth that, like the sped of an evil wish, the fever they create comes daily home to their own lying-in rooms. There have been State inquiries into cattle lung and mouth disease, scab in sheep, small-pox, diphtheria, typhoid, but none into the origin of puerperal fever. It is whispered about in family circles with bated breath, and a timid, feeble, fragmentary government dare not assail the source of danger. Nor has any publicist freely spoken out on the painful topic. So influential has been the popularity of the founders of the hospital that none have dared to challenge their position, so as to put a stop to the bad practice of combining midwifery wards with a surgical infirmary. In any other city such a combination with so fatal results would be disbanded in a week as a disgrace

to the medical profession. The subject was debated at the Medical Society of Victoria, and in the *Aust. Med. Jour.*, when the leaders in self-defence, and with phrase as homely as their etiology, assured the public that "the fever was owing to something in the air, and could not be helped." Nor is this the worst feature of the case. Some medical men, apparently aghast at the high rate of puerperal mortality, try to disguise the fact by registering some other disease as the cause of death.

But, returning from this not altogether irrelevant digression, if the proved spontaneous origin of puerperal fever be held as inferential proof of the probable spontaneous occurrence of typhoid fever, then assuredly will it be equally allowable to argue from one contagious fever to another, so as to elucidate their universal laws.

Thus stands the question between the great advocates of mere propagation of reproduced fever-seed on the one side, and of autogenesis on the other.

To presume to arbitrate between them would be the acme of temerity; and yet both sides have appealed to medical men living in thinly-

peopled new countries as having good opportunities of tracing individual cases of fever, either to other preceding cases or to the conditions of spontaneous origin.¹ Here, as in England, "The great majority, not of the laity only but of the profession also, still remain anti-contagionists. * * In the discussions on the cause of typhoid fever which filled so large a space in the public papers, both lay and medical, * * the idea of contagion in connexion with the disease was almost universally ignored or repudiated."² Whoever, therefore, would advocate a purely contagious source must meet with the strong opposition of deep-set prejudices, ingrained into minds whose earliest lessons in sanitation were got from Southwood Smith's fermenting - garbage theories; and whose systems seem unable to assimilate with fresher etiological facts. The foul drains, smells, abounding organic impurities, swamps, &c., have seemed to furnish sufficient miasms to readily account for every sporadic case or local epidemic of typhoid as of every other

¹ Dr. Budd, *Typhoid Fever*, p. 9, and *Lancet*, May 4, 1872.

² *Typhoid Fever*, pp. 5, 6.

form of prevailing fever; while a contagious poison, unseen, unfelt, and having no one physical quality appreciable by the aided or unaided senses, is more difficult to trace through its effects, which is the only possible mode of deducing the fact of its existence.

Nor is this doubt as to the cause of fever, and its right classification accordingly, confined to Victoria. Our neighbours in Sydney, who likewise suffer a severe death-rate from zymotic disease, as much as 26·23 per cent. of the aggregate mortality being from miasmatic fevers alone, attribute “the main cause of disease in the city to be accumulation of animal and vegetable matter in the low-lying portions of Sydney, to stagnant pools, and to the ill-constructed and badly-ventilated dwellings of the poorer class.” In his report for 1869, the Registrar-General endeavours to explain a misapprehension that there, as here, “exists as to the term ‘typhus,’ which is applied to fevers, and classed under the miasmatic order.” “The number of deaths,” he continues, “set down to that disease includes all those of a typhoid character.” * * “It does not necessarily

follow that typhus should be interpreted as referring to fevers of the malignant and infectious type, which, as far as my experience extends, are not of frequent occurrence; but when such cases do arise they would certainly be classified with those designated colonial, gastric, and other fevers." All this is most tantalising unscientific confusion. It may gratify, because concurring in the mixed views of, our own hygienic authorities; but while such unsettled opinions exert their influence, fevers never can be classified according to their causes; and this classification is the real desideratum of sanitary science at the present time. The class of zymotic diseases is in greater number in these two colonies than it is in England and Wales, where, in addition to simple continued and typhoid fevers, such as we chiefly have, besides the exanthemata, there are typhus, relapsing and intermittent fevers, and small-pox to swell the fatal list, which we have not; so that we here really appear to suffer more from typhoid fever proper in proportion to population than they do in England. In the face of these facts the medical section of

our Central Board of Health would assure the public that "the whole of the number of cases (of typhoid fever) which have occurred in this colony are not great (!) and notwithstanding that no measures have yet been taken for preventing its spread, it has not often been widely disseminated." *Qui s'excuse, s'accuse.* This explanation may suffice as an apology for the conscious remissness of our sanitary officials; yet, coming from a board of health, it sounds strange doctrine concerning a fever which, with due appreciation of its causes, could easily be reduced to the condition of a "pathological rarity."

If it be true, not only that typhoid fever is virulently contagious, but, also, that its spreading in no other way than by contagion is "the master-fact in its history," the fact deserves the fullest demonstration.

That typhoid fever is contagious may be inferred from the analogy of its leading phenomena with the phenomena of kindred contagious fevers. An attack of it secures its subject against future seizures. In conferring this immunity typhoid conforms to the laws of

small-pox, the typical contagious fever. The self-expending fevers caused by malarial infection do not so protect their subjects. Ague, hay fever, malarial remittents, may affect the same person many times; contagious reproducing fevers attack the same individual only once in a lifetime. The calenture (sun-fever), simple continued fever,¹ or febricula (not the abortive typhoid so misnamed), very common in hot dry

¹ The President of the Central Board of Health, at whose suggestion I began this investigation, and one of my judges who condemn the report to the Government as inutile, assures me that "*there are many forms of continued fever besides typhoid prevalent in Melbourne.*" On requesting him to name one, besides the ordinary simple continued fever, he said he often saw cases of gastric fever, some of them severe and even fatal. Upon reminding him that the term "gastric" is by all good medical writers employed as a synonym for typhoid, I then asked him if he meant a form of fever other than typhoid to which he applied the term "gastric," and on his replying in the affirmative, I begged him to define the form of fever he would so differentiate; but this he was, to my mind, wholly unable to do; and from this confusion I am constrained to think that he cannot yet have given sufficient attention to the details of this question to master the radical principles of the intricate subject. The improbabilities of any large portion of the fever death-rate having been due to Simple Continued fever may be inferred from the fact that in twenty-five years that form of fever was not once fatal in the London Fever Hospital.

seasons here, as in all countries, coming on abruptly, without any period of incubation, after fatigue and exposure to the direct rays of the sun, is excited by insolation or nerve-shock, according to the old medical doctrine of solidism; while typhoid and allied contagious fevers are induced by morbid poisons entering the blood, there incubating and reproducing themselves, at the expense of blood elements, according to the ancient humoral pathology. The action of these morbid poisons has been called catalytic; but catalysis itself has not been explained. It suffices to know that infecting poisons of specific expending fevers, whether existing in the form of living organisms, or as gaseous miasms, do not operate as contagious poisons act by direct mingling with the blood, neither do they through any known or unknown chemico-vital changes reproduce the agents of the communicable disease.

That there can thus easily be discerned, according to their proximate causes, two grand classes of fevers, the contagious and the non-contagious, should lessen the confusion of ideas about their exciting causes,

and remove the grounds of conflict about their diagnoses.

The difficulty of placing relapsing fever in either category may disappear with a fuller knowledge of the stave-like bodies recently found by Obermeier, and of the abnormal cells discovered by Ponfick,¹ in the blood of patients suffering from that disease. Relapsing fever is held by Dr. Murchison and some other writers to be contagious, yet liable to occur more than once to the same individual; but Virchow denies its contagious power. It certainly differs from other contagious fevers, in that it does not, like them, seize victims in all grades of life, but alone attacks the famishing. Consequently we, in this colony, have no clinical experience of relapsing fever.

That some radical change occurs in the fluids during acute specific fevers caused by the action of specific morbid poisons, to let them leave the body no more prone to repetition, is evident; and the fact has elicited efforts at explanation from some of the greatest medical philosophers,

¹ *Lond. Med. Rec.*, No. 15, et seq.

amongst whom are Simon,¹ Holland,² Paget,³ Liebig,⁴ Watson,⁵ Tyndal,⁶ Beale,⁷ Bastian,⁸ Carpenter,⁹ and Burdon Sanderson.¹⁰ Physiologists admit the presence in the blood of component ingredients of unknown purpose in the economy; elements in no way essential to life, since, after their removal by conversion into fresh zymotic poison, the mass of blood remains as vital as before.

Analogy may perhaps explain the paradox. Rudimentary organs, large in foetal life, are early removed, or reduced by absorption to the cellular matrix; and the elements, after the organs have served a transient function in embryonic evolution, linger superfluous in the blood, furnishing pabulum for elaborating morbid poisons by processes called fever.

¹ *Lect. Path.*, p. 262.

² *Dis. Occurring Once in Life, Med. Notes, &c.*

³ *Surg. Path., Specific Disease.*

⁴ *Lett. on Chem.*, let. xx.

⁵ *Lect. on Med.*, vol. ii. p. 859, 1871.

⁶ Cited in above, p. 859, vol. ii. 1871; and also in *Dust and Disease.*

⁷ *Disease Germs*, 1872, passim.

⁸ *Begin. of Life*, vol. ii. App. E., p. cxxvii. et seq.

⁹ *Prin. Phys.*, 7th ed. p. 251.

¹⁰ *Twelfth Rep. Priv. Coun.*, p. 255.

This is Simon's view; and the theory explains the insusceptibility to some exanthems of early infancy, while rudimentary organs are not yet wholly absorbed,¹ nor the blood charged with waste products for morbid ferments to work on. So, when puerperal blood holds absorbed elements from the involving womb, and is loaded with excrementitious material undergoing reducing changes prior to excretion, a state of blood exists similar to that which contains the waste matters of foetal life, and makes it prone to rapid zymotic action.² This is why women are not liable to fever till after confinement.³

If, therefore, typhoid fever observes the conditions of true zymotic fevers in all other particulars, it may likewise be inferred to obey the ordinary laws of contagion.

To assert, as some do, in a compromising spirit, that typhoid fever is only contagious in a

¹ "The thymus gland is sometimes found unabsorbed in adult, and even in very aged animals."—Chauveau, *Comp. Arrat.*, p. 473.

² Simpson, *Dis. Women*, p. 352; Hudson, *Lect. on Fev.*, p. 15.

³ Leake, *Puerp. Fev.*

limited degree, is ambiguous, unscientific in theory, and perilous in practice.

But even, although it were no longer logical to argue from one form of specific fever to another, nearly every one of these obeying its own special laws, as Dr. Murchison contends, there is yet one generalisation applicable to all fevers, which is of prime importance in deciding between theories of contagion and non-contagion. And it is well worth considering, because of the ambiguity constantly occurring in the use of the terms "infection" and "contagion." Professor Huxley, in one of his popular lectures on this subject,¹ speaks of some diseases as being both infectious and contagious; yet, wherein lies the difference he does not even try to explain; and such is the case with almost every one who thus loosely uses the expressions. The popular and the medical notion is of the same vague sort, as if the difference were solely in the mediate or immediate mode of communication. But a deeper pathological distinction of great significance in theory and in practice should be

¹ Critiques and Addresses, *Biogenesis and Abiogenesis*,
p. 244.

drawn between infection and contagion. In the former there is in the infected subject no reproduction of the infecting virus; in the latter the virus, or morbid poison rather, is reproduced within the sick body. Further, an infecting virus is invariably a product of normal growth or secretion, and is therefore always physiological in its nature; whereas a contagion virus is as constantly an abnormal product resulting from a morbid process, and is therefore always pathological. Infections begin in healthy function, contagion in diseased function. Infection is physiological, contagion pathological. Serpent venom, the ague plant, if it exists, and the pollen of hay—the exciter of hay-fever—are examples of infecting agents never reproduced within diseased or infected bodies, but known only in independent existence external to these bodies. On the contrary, vaccine virus, syphilitic, variolar, scarlatinal, typhoid, and typhus poisons are examples of contagious agents—true *contagia* — alone elaborated within diseased bodies, having no other known source, and entirely unknown as entities except in connexion with diseased bodies. These meanings

differ from the primary or etymological sense of the terms ; but they are the best medical definitions consistent with known facts, and, if invariably adhered to, would remove a great source of confusion from both lay and professional minds, and tend to clearer views on the causation of specific and non-specific fevers. The confusion is the more surprising, after the lucid exposition of Anglada's ingenious views in Trousseau's clinical lecture on contagion, a chapter which seems to be too little read.

Exception may be taken by some to the statement that serpent venom is not reproduced, for Dr. Fayrer states that the blood of a poisoned animal poisoned other animals, and in this way descended through three bodies at least ;² but it is not shown that the venom was reproduced, or that the poisoning could go on *ad infinitum* without returning to the original source of supply. The above views are certainly opposed to Dr. Murchison's views as to the reproduction of the poison of typhoid in fermenting excreta.

² *Thanatophidia*.

But the tenability of that opinion is doubted by many of the best authorities ; while, if the axiom enunciated by Sir T. Watson be true, that *contagium* is never reproduced save within the sick body, the theory of pure contagion would be confirmed, and the distinction drawn between contagion and infection be made valid.

It may be worth a passing allusion, that Contagionists usually apply their arguments directly and finally to mere affairs of practical hygiene, to the destruction of *contagia* and arrest of contagious diseases, as the end and aim of their science and art ; while many leading evolutionists, or non-contagionists, or partial contagionists, generally mix up the debatable problems with an underlying doctrine with which health or sanitary affairs *per se* have nothing whatever to do, but which solely concerns abstract or transcendental philosophizing about the origin of living matter. When Dr. Burdon-Sanderson was asked to verify some experiments of Dr. Bastian's, originally designed to test the germ theory of the origin of specific fevers, he at once disclaimed all intention of entering into the controversy

on spontaneous generation.¹ Contagionists are satisfied to free a people from diademics of preventible fever; non-contagionists would add a teleological discourse on life theories and religious thought.² "It seemed to me that no real advance could take place in our power of controlling these diseases until certain other great problems had been settled."³

When bad effects are generally and vaguely attributed to organic impurities, putrefactions, fermentations, miasms, want of ventilation, ochlesis, and such like asserted causes of specific fevers, it is never explained how the same causes engender diverse diseases. So long as specific contagion had been excluded, we, in this colony, have never witnessed the origin *de novo* of any one specific fever. Every one of these diseases that has hitherto come amongst us has had its specific germs or seed imported. No synthetic skill can devise the combined influences that will generate, without the agency of specific poison, the poison of any

¹ *Nature*, 9th Jan. 1873.

² Beale, *Life Theories and Religious Thought*.

³ *The Beginnings of Life*, vol. ii. p. cx App.

fever as yet a stranger to the colony. Whether to include typhoid fever in the category, or whether it be sometimes due to malaria, as would almost appear from the seasonal accession, may be fair subject for debate ; but in the discussion carefully observed facts should be adduced as well as theoretical opinions. It is superfluous to add that the point is highly important to determine, as the only guide to the direct means to be used to prevent that fever. These means may be devised, as some aver, to "cover both issues," and the theory would be of less importance if the practice were good, as it certainly is not as far as preventive measures are concerned, for the prevalence of the fever inclines to increase rather than diminish.

In some observations already cited, on fever in a mining township, proofs of contagion were said to be exceedingly rare, the observer evidently believing in the greater influence of drain miasm. Altogether, little was said of etiology. For, "although enteric or typhoid fever is so common and fatal in Australia, very little has been published on that subject by

colonial practitioners." The cause of fever has seldom been debated by the medical societies here; and, as far as it has been considered, the prevailing belief seems to have been against the contagiousness of typhoid. It is, therefore, little surprising that people have been put off their guard and have allowed fever to take deep hold on the community. Thus much for loose theories in medical science. On one occasion one of the earliest settlers brought his long experience of disease in the colony to bear on the origin of typhoid. His view was, that prior to 1852-3-4, the era of gold-mining, a non-contagious fever of remittent type prevailed, and that as settlement went on that type of fever disappeared, and was succeeded by true typhoid. "When the gold-fields attracted so vast a number of persons to this colony in the years 1852, 1853, and 1854, human beings had to huddle and crowd together, and hygienic measures were so utterly disregarded, that another disease" (other than colonial fever), "a preventible one, although it had occasionally been observed years before, broke out, was very

violent and much more fatal, namely, typhoid fever.”¹

The cases described in illustration, although spoken of as “bilious-remittent”² bore close

¹ *Aus. Med. Jour.*, June 1869, p. 181.

² “Under this vague term, ‘*bilious-remittent fever*,’” remarks Professor E. A. Parkes, “a disease or diseases, which in many points are like relapsing fever, but yet are not identical (Marston), have been described as occurring especially in Egypt (Griesinger), and in the Levant generally. The exact causes are not known; but in some of the writings of the older army surgeons, the fevers which are produced by foul camps (in addition to typhoid) appear to have a close resemblance to the bilious-remittent fevers of the Mediterranean. They appear to be connected with bad sanitary conditions, but their exact causation is not clear.”—(*Pract. Hyg.*, 1873, p. 454.) The confusing force of this “*vague term*” is best seen by bringing the above remarks beside the observation of Dr. Murchison, “that some writers have confounded tropical yellow fever, or ‘bilious-remittent fevers’ of malarious origin, with relapsing fever. If the ‘bilious-remittent fevers’ of malarious origin, the jungle fever, and ‘tropical yellow fever’ be synonymous, then, according to some of our local authorities, we must have yellow fever endemic amongst us. It is asserted that, in the absence of marsh malaria, or paludal causes of any description, the volcanic soil upon which some parts of Melbourne rest, would sufficiently account for the occurrence of “bilious-remittent.” But, to ascribe the same fever both to “bad sanitary conditions” and to a “volcanic soil,” to two causes for one effect, appears to be a somewhat loose and illogical method of induction. “There can be no doubt that enteric fever and the

resemblance to genuine typhoid, in the long indefinite illness at the onset, the shivering and peculiar headache ushering in the second stage, which lasted often twenty days, with great depression, morning remissions and evening exacerbations, often with persistent diarrhœa and hæmorrhage from the bowels, and frequently without marked crisis, but with protracted convalescence. No instance of *post mortem* inspection is mentioned by which any doubt about diagnosis would have been set at rest on that occasion.

No hint is even suggested of the possible importation of the contagion, although it is a well-known fact that many ships were about that time placed in quarantine on account of having fever on board, many of them having suffered severely, as will immediately be more fully detailed.

As a matter of almost absolute certainty, both typhoid and typhus fevers must have

adynamic forms of remittent have often been confounded in India, and yet the diagnosis, to a careful observer, is not difficult," observes Professor M'Lean. Have they been equally confounded here? or is the diagnosis more difficult here than in India?

been imported, just as certainly indeed as that scarlatina, measles, hooping-cough, &c., were so brought amongst us. That typhus should die out, and typhoid not, could readily be explained by considering the different ways in which these two fevers are propagated—the former by direct personal communication of the poison, exhaled from the lungs and skin, the latter through the intermedium of the fæcal-poisoned soil. “It has been credibly reported to me,” observes Dr. Budd, “that typhoid fever was quite unknown in Queensland until it was brought there from England in the *Flying Cloud*, a fever-stricken emigrant ship. Since that date it is said to have become established in the colony. For obvious reasons, I am unable to verify the statement; but if the circumstances really were what they have been represented to me to have been, they would be well worth inquiring into in relation to the question before us. Some record of them is, no doubt, still to be found in Brisbane.”¹ For the

¹ *Typhoid Fever*, p. 172.

settlement of the question relating to our own case, the matter certainly deserves inquiry.¹

In an article on the fevers of the Gulf of Carpentaria, it was stated by the author, who was residing in the northern colony, that the continued fever seen there seems to have been a diademic of typhoid, introduced into the Gulf districts by the *Margaret and Mary*; and was the disease which caused the death of so many of the northern pioneers, and unfortunately stamped the north of Carpentaria as a dangerous fever district. There was no doubt about its being the *febris typhoïdes* of Louis, as the daily recurrence of the diagnostic "*taches rosées lenticulaires*" was observed.²

Another proof of the importation of typhoid fever was given a few years ago in a presidential address to the Medical Society of Victoria.³ The speaker stated that typhoid fever first appeared in Victoria in 1842. Three men

¹ In the *Narrative of Missionary Enterprise*, by the Rev. Thomas Williams, it is asserted that "most of the epidemics that raged in the islands were brought there by ships."

² *Aus. Med. Jour.*, Dec. 1867.

³ *Aus. Med. Jour.*, March 1865.

from an immigrant ship (the *Salsette*) that had fever on board were engaged as farm servants. Two of them died of fever, and others about the farm were attacked in a similar way, thus leading one to suspect its contagious nature. Many more of the immigrants who went far into the bush were also similarly attacked, and communicated the disease to their fellow workmen. This is very important testimony. Doubtless several of those cases of fever in the country were accounted sporadic, and were supposed to have had spontaneous origin *de novo* in surrounding putrescence of organic matter. The oft-cited argument about new comers being more susceptible than older residents to fever-causing miasms did not hold in that instance, for fever was on board the vessel which the sick persons had left, and they doubtless had fever incubating at the time of their landing.¹ The President also said that

¹ "See you not too that all who come to a place far away from country and home are affected by the strangeness of climate and water," says Lucretius, and sometimes they carry his famous "seeds of fevers" with them.—(*Munro's Trans.*, 3rd ed., p. 179.)

"Since resuming practice in 1860" (after a lengthened visit to Europe), "I have not seen a genuine case of the old colonial fever. Typhoid fever seems to have taken its place," adding the remark that "pulmonary consumption is greatly on the increase, and runs its course rapidly to destruction."

Again, in a paper read to the Medical Society,¹ on some cases of typhus and typhoid fevers admitted into the Melbourne Hospital, it was clearly shown that both fevers had been imported—typhoid by the *Sam Cairns*, from Liverpool, in October 1864, and typhus in January 1865, by the ship *Golden Empire*. The cases were minutely discussed to differentiate the diagnosis, and all concurred in the fact of their importation. This point is thus urged, because unless clear proof be adduced of carried contagion, the alternative is to admit spontaneous origin. If the fever so arose *de novo*, once, it may so arise *de novo* every day out of the like conditions. But if carried contagion can be shown to

¹ *Aus. Med. Jour.*, March 1865.

have brought the disease here in the first instance, the greater becomes the probability that it is always so propagated, and is never in any sense sporadic or endemic, and that the only rational practice of hygiene is to destroy the portable poison.¹

It has often been stated that typhoid did not exist in Tahiti until after the discovery and colonization of that island by Europeans. And "Now, it is reported, the population is being rapidly swept off by two principal scourges — typhoid fever and phthisis, the latter, also, as some believe, imported from Europe."

Similar results appear to have attracted attention in America, where the opinion is held that enteric fever and phthisis appear after the cessation of intermittents, following the drying up of marshes and increased cultivation.² But Dr. Murchison thinks the facts suggest a similarity rather than an antagonism between enteric fever and malarious fevers, the poisons in both

¹ See *11th Rep. Cent. Bd. Health*, p. 5.

² *Cont. Fevers*, p. 451.

instances being generated under similar circumstances. By the census of the United States, the deaths there from phthisis in every 100,000 living were¹—

In 1850	144.5	deaths.
„ 1860	156.1	„
„ 1870	181.3	„

Something very nearly analogous has taken place in this colony. The progress of typhoid has been already shown; and as for the extent to which phthisis has increased, some idea may be gained from the comparison of the last with the former census returns. Thus, last census, 1871, showed 43,000 persons in the colony between the ages of 20 and 35 years fewer than in 1861, and decreased total deaths from phthisis; yet the ratio of such deaths to numbers living, at those the most consumptive ages, was increased 11.6 per cent. For, in 1871, 212 persons between the ages of 20 and 35 years died of phthisis. If the population at those ages had continued the same as in 1861,

¹ *Census Statistics*, U. S.

instead of decreasing by 43,000, deaths from phthisis would by calculation have been 274, or 62 more than actually took place. "The death-rate from phthisis, in relation to the whole population, is very much lower in Victoria than it is in England and Wales; it must be remembered, however, that the population at the ages most liable to phthisis has decreased during the decenniad, and, therefore, the complaint is really more fatal now in Victoria than it was some years since." ¹

It is usually thought that Australian climates, in all their minor varieties, act beneficially on phthisical persons, by reason of their warmth and dryness. But, singularly enough, the greatest mortality from phthisis has, with the sole exception of August last, when the deaths for the month from phthisis numbered 60, the greatest number yet recorded in any one month always occurred during unusually dry and hot seasons. Thus, in March, 1870, a hot and dry month, at the

¹ *Govt. Stat. Rep.*, 1874, p. 53.

very close of a long drought, when everything in nature was arid, scorched, or burnt up, there were, in Melbourne, 51 deaths from phthisis, this being then the highest monthly mortality ever recorded of that disease in this city; while in December 1873, which was the first very hot and dry season for a long time, the periods intervening between then and March 1870 having been mostly moist open seasons, the death-rate from phthisis rose, for the first time since the last hot dry season of 1870, to over 50, there being 53 deaths from phthisis in a total mortality for the month of 502. The combined conditions of warmth and dryness did not, therefore, during those seasons, seem favourable for the phthisical. What is true of Melbourne has been ascertained to be equally true of the whole colony. Phthisis and typhoid would seem to be, as a rule, injuriously affected by dry hot weather in this country.

At the present time the chief representatives in Britain of the two opposing views of causation in typhoid are Dr. Murchison and Dr. Budd, and each has quite recently

published a large monograph more fully and firmly than ever setting forth and maintaining their well-known opinions; both of which works they who would master the subject must carefully peruse. Without this perusal, those who have to direct the sanitary regulations of Melbourne for the extinction of typhoid fever, will be at great disadvantage. They will have to remember that Dr. Budd affirms that a specific poison is reproduced in the glands of the intestines; that this typhous matter is produced in no other way, and that it is cast off with the alvine discharges; while Dr. Murchison, not denying that enteric fever is in some way communicable by the sick to the healthy, or even that the alvine dejections appear to constitute the chief if not the sole medium of communication, says it does not follow that the sick give off from their bowels a specific virus like that of small-pox, but that all evidence is in favour of the view that the fresh evacuations are harmless, and that the poison is developed during their putrefaction. In short, Dr. Murchison argues that enteric fever-poison is generated by fermentation of

fæcal, and perhaps other forms of organic matter, whether that fæcal matter comes from healthy persons or from persons suffering from typhoid fever. The poison, according to him, is formed in fermenting stools, and the pre-existence of typhoid fever is immaterial, except that typhoid discharges, being alkaline, and not acid, like healthy motions, are very prone to decomposition and putrefaction, and in this way more apt to propagate fever.

In this glorious contest between Achilles and Agamemnon, about the Briseïs, Typhoid, it is not for me to aspire to act the lieutenant's office of Patroclus, although, like that staunch henchman, I have sustained, and I hope parried, from Hector what was meant to be a fatal thrust; yet, waiving all experience of my own, may not I interpose between the combatants the matured judgment of one of the Nestors of our profession, delivered in favour of my chosen side? Professor E. A. Parkes, after duly and unbiassedly weighing all the evidence, and making every allowance for "*difficulties*," gives it as his final decision that "it seems clear that the theory of a specific cause reproducing itself

in the intestines and contained in the discharges, and naturally, therefore, connected more or less closely with excreta and sewers, and sometimes with drinking-water, is that which best meets the facts which have been most faithfully reported in outbreaks of typhoid fever."

"Entrance by food (milk) has been lately also proved." Then, again, alluding to the larger question of the origin and propagation of specific poisons, he asks, "will decomposing sewage in water produce typhoid fever, or must the evacuations of a typhoid patient pass in?" and the same high authority answers, that "though polluted with excrement for years, no enteric (typhoid) fever appeared until an imported case introduced the virus. Positive evidence of this kind seems conclusive, and I think we may now safely believe that the presence of typhoid evacuations in the water is necessary. Common fæcal matter may produce diarrhœa, which may perhaps be febrile, but for the production of enteric (typhoid) fever the specific agent must be present." And again, "all the conditions of accumulated sewage, &c., which are supposed

to produce typhoid fever, were present for years, and yet no fever resulted. Then a patient came from a distance with typhoid fever, and the disease spread through the village, either through the medium of the water (as is perhaps most common), or through the air. These instances are so numerous that the entrance of a fresh agent must be admitted, and if so, the series of events becomes quite intelligible. * * The doctrine that a specific cause is necessary for the production of typhoid fever * * * will be found to explain almost all the events which have been recorded in connection with the origin of typhoid fever."

"It must, however, be remembered that the carriage of the 'contagion' takes place in so many ways that it is impossible always to trace it. * * * The disease itself is also often so slight that persons move about and believe they have only an attack of diarrhœa. * * Again, the frequent journeying from place to place exposes all persons to a greater chance of inhaling the typhoid effluvia, and the real source of the disease may be

far removed from the place which is actually suspected.”¹

If, therefore, to the list of names already given, of men illustrious in the annals of medical science, who, endorsing and upholding the doctrine, first enunciated by Canstatt, Gietl, and Budd, of a specific virus formed in the affected glandular structures of the ileum, constituting the furuncular disease, or eruption of boils on that part of the mucous membrane of the smaller intestines, the true *dothiènentérite*² of Bretonneau;—if, let me repeat, to those names, so famous, may be added the name of the author of a work that is universally held to be the best text-book on public hygiene ever published in the English tongue, in support of the opinion offered in these pages, that the spreading of typhoid fever is entirely and alone owing to one common all-sufficing cause—contagion; and is not due, as is by the general voice of the profession affirmed, to a precarious poisonous ferment, assumed without actual proof to be formed by the chance brewing of

¹ *Practical Hygiene*, 4th ed., 1873, pp. 46, 47, 123, 124.

² Δοθιήν, a boil, and έντερον, a piece of the guts.

seething mud, in other words, that it is not a '*schleim-fieber*;' assuredly I may now most freely leave the final demonstration to future research, and quietly disregard all minor frivolous fault-findings that have been essayed in disparagement of my work, and against the chain of arguments by which, in the execution of an ungracious, thankless task, I have sought to establish my position. The truth or error of these conclusions may mean life or death to thousands of people in this colony.

In ignoring the potency of dirt, filth, and all impurities, too familiarly surrounding the homes of bestial humanity, for creating a specific fever like typhoid, I well know the dangerous attitude. For the denial offers a target to the satires of those holding different views, who fling jibes at all whom they falsely accuse of pleading for filth, fond of dirt, apologists of stinks, and who care not to have garbage removed, and cleanliness and decency enforced among the slothful of mankind. This false or irrelevant conclusion — this *ignoratio elenchi* about dirt and disease, is constantly thrown in the teeth of advocates of contagion,

by derisive opponents without stronger arguments to wield. But I can at once reassure those who would adopt the view of a purely contagious causation of typhoid fever, that the theory by no means implies or involves carelessness about dirt and disease. Dirt creates disease in many ways, although it is neutral about one particular mode. These dirt-engendered diseases are often deeper, more deadly and enduring, with worse remote consequences, than even the fatal out-spoken fever. All hygienists know that children reared amidst foul air are stunted, blanched, etiolated, sickly, melancholy, dwarfed in mind and physique; and that adults living, or rather struggling for life, in unwholesome atmospheres, often made so by civic rulers' pride of arbitrary office, become dyspeptic, nervous, irritable, wan, feeble, irresolute, craving for stimulants to conquer the langour of vital depression following deprivation of wholesome invigorating stimulus so amply provided in pure air by nature, whose bounty it is the function of boards of health often to cancel or destroy. If myriads of oxygen-devouring organisms,

acting as nature's scavengers, for removal of waste organic matters, become competitors for oxygen with men, then must the latter, numerically weak, be robbed of their allotted share of the first vital element of nature. To this argument will be offered the reply that diffusion of air is so rapid, instantaneous, and all-pervading, that, theoretically, no deficiency of supply ever can happen. But this is the physicist's theoretical mistake. Actual tests applied in careful experiments, recorded in such treatises as Dr. Angus Smith's work on *Air and Rain*,¹ and also observations of the natural phenomena of ozone distribution and the causes of its perturbations, in works like that of Dr. Cornelius Fox, *On Ozone and Antozone*,² clearly demonstrate the poverty of air for human use wherever much organic putrefactions go on. The factories of life and the workshops of death may be carried on side by side as the mutual complements of each other, but both kinds of work cannot be conducted at one and the same time under the same roof. In that

¹ *Air and Rain*, 1872, p. 41.

² *Ozone and Antozone*, 1873, p. 162.

struggle for existence man has always the worst of it, all *dilettante* assertions of mere pneumatic physicists to the contrary notwithstanding. Therefore, it is still, although for widely different reasons, for sake of wholesome air, as imperative upon the custodians of our health and the lungs of our cities, to sweep away dirt, and make none themselves anew, and keep all corners about human dwellings, and open spaces in public parks, as clean and sweet as if that oxygen-devouring dirt did really create the special pestilence that is alone propagated by contagion.

The very great importance attached by the author of the pythogenic theory of typhoid fever, to the alkaline state of typhoid evacuations, on the assumption that because being of that chemical composition they are more prone than normally acid healthy stools to decomposition and fermentation, as a "condition that must be fatal to the existence of an animal poison, such as that of small-pox," or indeed of any specific virus, other than the poison generated during their own fermentation, seems scarcely to rest upon adequate reasons.

That typhoid stools are alkaline is probably owing to an increased flow of the normal alkaline secretion of the mucous follicles. Professor Goodsir always found the villi and follicles of Lieberkühn to be healthy.¹ Why such a condition, even with the detritus of sloughs from the mucous membrane covering the solitary and Peyer's glands, should be fatal to the presence of an animal poison, such as the specific typhous matter, in the *mucous exanthema* of the ileum, the '*atriis morbi*' of typhoid fever,² is not quite clear.

In the numerous experiments conducted by advocates on both sides of the grand controversies on heterogenesis, acid solutions have been found the more prolific of every form of infusorial life. Indeed, the formation of an acid was held by Schræder and Dusch to be the cause of fermentation.³ The *proligerous pellicle* of Pouchet formed freely on acid solutions. *Penicillium* grows on milk after acquiring an

¹ *Anatomical Memoirs*, vol. ii. p. 373.

² Rokitsky, *Path. Anat.*, vol. iii. p. 57, Syd. Soc. Trans.

³ J. H. Bennett, *Text Book of Phys.*, 1872, p. 432.

acid reaction.¹ "It should not be understood that we regard all acid solutions as having a low evolutionary or fermentative tendency. On the contrary, evidence has already been adduced to show that *some* acid solutions are most prone to undergo evolutionary changes of a certain kind. These do not result in the production of living things of a high type, but rather in an abundance of organisms of a comparatively low type."² Again, when the bodies termed '*micrococci*' by Professor Hallier are introduced into an acid fluid, or fluids becoming acid by a new kind of fermentation, they develop into *bacteria*. The alkaline vapour of ammonia prevents the fermentation of alkaline milk as well as the putrefactive changes of acid muscle. The buccal and nasal mucous membranes, naturally alkaline in health, form the *nidus* of infusorial life development whenever their secretions turn unhealthily ascendent. This acid state is thought to be essential for the receptivity of the pollen of hay, to excite the hay fever, *catarrhus æstivus*, by liberating the

J. H. Bennett, *Text Book of Phys.*, 1872, p. 439.
Begin. of Life, vol. i. p. 391.

zoöspore encased in the pollen grain and developing it into *bacterium*, an organism entirely different from that into which it would have grown in its destined matrix in the floral ovary. Therefore, although in these analogies the new infusorial bodies may appear only as *epi* and not initial phenomena,¹ they yet afford some argument against the view referred to, either that alkaline typhoid stools are inimical to the existence of a specific virus, such as the animal poison in vaccine lymph; or that they are more active in exciting typhoid fever by their greater tendency than acid normal stools to pass into putrefactive fermentation, and to generate the peculiar fætid organic vapour, a subtle poison² of unknown chemical constitution, more or less present in sewers, and sometimes termed *typhine* or *typhoidine* by the advocates of pythogenesis.

It is remarkable, and perhaps worthy of note, that the mucous secretions of the ileum should be thus alkaline in typhoid fever, and yet be highly acid, like the rest of the alimentary canal,

¹ A. Mangin, *Études Zoologiques*, p. 18.

² B. Latham, *Sanitary Engineering*, 1873, p. 204.

in acute tuberculosis, two diseases often presenting closely corresponding symptoms during life, and also several almost identical morbid appearances after death, requiring on the part of the physician during life, as well as from the morbid anatomist, the greatest acumen in the differentiating diagnosis.

Indeed, several authors refer the typhous matter which is formed in the small glands of the intestines in typhoid, and described as the specific virus of the disease, to the same category as the *granulie*, or granular tubercle of phthisis, thus showing another analogy between typhoid fever and acute tuberculosis. The latter affection has even been pronounced by some authorities to be a true specific fever, with a specific virus deposited in the adenoid or glandular tissue, which is extensively diffused through the submucous layer of the air tubes of the lungs, this being the true anatomical seat of the so-termed lung fever, just as the agminate glands form the anatomical seat of typhoid. Both the intestinal and the pulmonary lymphatic glandular structures are analogous in physiological constitution and in function,

and both are the seat of a specific typhous deposit as the anatomical expression of two closely similar fevers. In actual practice acute tuberculosis is the one disease most apt to be mistaken for typhoid fever, and *vice versâ*. The two widely-different maladies have thus many singularly striking points of resemblance. Moreover, tuberculosis is prone to be in youth the one special remote sequel of typhoid fever in early life, of which in this colony many examples have come under my own notice. By this remark I do not wish it to be inferred that I would endeavour to trace any affinity between the two fevers, although even that is by no means improbable.

A new difficulty here presents itself. It is disputed whether the intestinal glands affected in typhoid are excretory, or lymphatic and absorbing, and the best physiological authorities are cited on either side. If the former, these glands, in typhoid fever, may eliminate the specific poison—as some affirm—from within; if the latter, then may they only absorb a poison from without, as is asserted by many others. The subject is indeed

beset with intricacies at every turn, and much needs "the more ample discussion" to which Dr. Budd hopes, on some future occasion, to subject it. Then will that inquirer closely scrutinize the nature of that dark granular matter in the mesenteric glands mentioned by Rindfleisch,¹ the precise nature of which is unknown, and which therefore may have some genetic connection with the specific poison of typhoid. These glands are always enlarged in the fever. Are they so from mere irritation of matter absorbed from the inflamed Peyer's and solitary glands as, so to speak, sympathetic buboes, or filled with granules of a specific poison? The large amount of protoplasm in the "typhous" cell of Rindfleisch seems to correspond to the increased bioplasm of disease germs described by Beale. Both lack durability, and alike soon pass on through necrobiotic, or life-decaying changes.

The existence or not of a specific typhous cell as a fever-poison will alone be ascertained by investigations like that proposed to be carried

¹ *Path. Histology*, vol. i. p. 441, New Syd. Soc. Trans.

out by Dr. Budd, with the improved means of such research now available. The poison resulting from the fermentation of fæcal or other organic matter must either be gaseous or solid. If the latter, its nature can perhaps be resolved by the higher powers of the microscope now in use among good pathologists. If the objectives of the $\frac{1}{50}$ inch, or even $\frac{1}{75}$ inch, fail to determine the precise nature of these minute particles, then is further research hopeless, save as methods of merely inferring their presence from their effects; and this is all that really has been achieved as yet. As we have been assured, on high authority, that "no connection can be traced between the numbers of bacteria, spores, &c., present in the air and the occurrence of diarrhœa, dysentery, cholera, ague, or dengue, nor between the presence or abundance of any special form of organisms or of cells and the prevalence of any epidemic disease,"¹ so may we be presently assured of the absence from the air, the blood, or the

¹ *Lond. Med. Rec.*, p. 17, 1874; see also *Pop. Sc. Rev.*, April 1874.

intestinal glandular structures, of a specific typhous cell, or other organism, forming a special typhoid poison. But if any such lower organisms exist, and should the now enormously increased microscopic powers reveal them, their source and nature may become known, also the means of destroying them, and of reducing typhoid sanitation to rational methods. "There is," observes Dr. Burdon Sanderson, "good and scientific grounds for anticipating that the solution of the problem will some day be attained by investigation of the morphological phenomena which attend the infective process. I am myself strongly impressed with the conviction that if this investigation is to be successful it must be experimental in every stage of its progress."¹ But is not, it may be asked, every well-observed case of typhoid fever, where the source of the cause is fairly traced, itself an experiment? Awaiting the more refined eventuality of pure science, may not such a degree of certainty as shall suffice for all practical ends,

¹ *Priv. Coun. Rep.* 12th, p. 242.

be reached by studying ordinary instances of typhoid fever? "I feel myself entitled to the credit of having done my best to penetrate into the anatomical essence of the typhoid process, with the aid of the most recent methods of investigation. The results, however, have not been equal to my expectation." Thus wrote, within the last three years, one of the most advanced of European histologists, Rindfleisch, whose labours in the same direction are still proceeding.

"The definite action of causes is as much the basis of medical science as it is of any branch of human knowledge." Results never come of chance, variation, or caprice. Causation is ever conversion of force; whether from the physical into the vital, or not, it is always in a definite direction. The same stability forms the basis of public hygiene. That "the morbid poisons act in all instances, not capriciously, but according to certain definite and specific laws,"¹ is the axiom of the contagionist doctrine of a continuous succession in acute fevers. The

¹ *The Morbid Poisons*, p. 22.

non-contagionist affirms a fortuitous causation, with capricious varieties of fever types, of which it is impossible to predicate anything, foresee the future reflected in the past, anticipate results, or guard against them by care or forethought.

Whether *contagia* are particles of matter thrown off from diseased bodies to similarly affect other healthy yet susceptible bodies, to be reproduced in them, and descend, like the bodies they infest, from generation to generation in unaltering specific form, through all time; or spores of fungi, each the cause of a special form of fever; or living organism of the lowest form of animal or vegetable life, belonging to the intermediate, or *protistic* kingdom of Hæckel, capable of rapidly multiplying within the blood and tissues, that is, in the bioplasm, protoplasm, or germinal matter; or some form of degenerating bioplasm imparting its mode of change to whatever healthy bioplasm it comes into contact with; or non-living albuminoid, acting as a dead-matter ferment, or catalytic; or organism engendered *de novo* by isomeric re-arrangement

of molecules of organic, or, perhaps, even of inorganic stuff, as some aver; or whether there be really no such disease-germs, or *contagia*, or poisonous particles at all, epidemics occurring from occult states of air of which nothing certain is or can be known, are among the moot points receiving, along with the physical and the vitalistic theories of life, keen discussion in every inquiry into the nature and origin of zymotic fevers. As long as these points are undecided they may be studied along with facts forced on the attention of those who are continually being brought into personal contact with individual cases of fever.¹

It may by some be thought that in a practical inquiry of this kind such abstruse questions of causation are out of place; while by others they may be pronounced inadequately investigated. There are, in this city, a few zealous inquirers into the origin of miasmatic diseases, who attribute the efficient causes to algoid and fungoid spores. By these students it may be asked why more importance has not been

¹ See *Aust. Med. Jour.*, July 1872.

attached to the proofs of the constant presence in the blood of vegetal organisms in almost every form of fever? why the testimony of many of the ablest men in medical science, to the invariable presence, either as cause or effect, of bacterial living filaments in the blood of persons suffering from such fevers as typhoid, has not been adverted to? or why no reference is made to the statement of Dr. J. H. Salisbury, that a peculiar algoid vegetation develops itself upon the external surface of the entire body, and upon the mucous membranes of the interior cavities, which he regards as the efficient cause of typhoid fever, and by means of which it is propagated? If verified by future researches this would be an invaluable discovery, disproving the supposed specific typhous cell theory, although still testifying to the contagious or communicable character of the fever, but in modes hitherto undreamed of. It is for our mycologists to investigate this important subject. Meantime, without ignoring its possible truth, for practical ends, it may be assumed that in a more easily explained way of transmission

the law that *omnis febris ex febre* holds good in typhoid.

Still, with the means of testing the validity of the doctrines of the nature and mode of origin, causes, or special laws of the etiology of zymotic diseases, of the theoretical views of the mode of action of the subtle morbid poisons, even of the physiological essence of the pyrexial state itself, it becomes a laudable aim to use them for so excellent an end. It would be so even although, as Helmholtz observes, "we bestow on it unwearied diligence for purely ideal purposes, without any approaching prospect of the pure science becoming of practical value."¹

"Such discoveries are not made by practical men, and they never will be made by them, because their minds are beset by ideas which, though of the highest value from one point of view, are not those which stimulate the original discoverer."²

But the inquiry is not wholly abstract and merely medical. It enters into a wide range of material usefulness. This consideration claims

¹ *Pop. Lect. on Scien. Sub.*

Prof. Tyndall, *Lect. on Light*, p. 218, 1873.

for it a hearing far outside the limited circles of the medical profession. It is fitting, says Dr. Murchison, that both the public and the profession should have just ideas upon the subject. The old long-debated, always-fresh, problem whether specific fevers ever spring *de novo* from mere physical surroundings inimical to healthy animal life, or are invariably derived from imported contagion, is not confined to the origin of the specific fevers alone affecting human beings, but equally embraces the causation of specific fevers incident to brutes, and therefore doubly concerns Victorian colonists, personally, as well as in their greatest source of national wealth. It is one of the touches of nature that makes the whole world kin: "*morbida vis hominum generi pecudumque catervis.*"

Few have the skill, and fewer the means of using the higher aids to pathological research; but all who daily see the practical aspect of acute specific fevers must form some notion of their nature and essence. Men of leisure and trained skill, working in the centres of scientific culture, amid refined philosophical thought, in laboratories replete with apparatus,

with minds constantly bent on interrogating nature, free from interruption of other duties, are, after prolonged research, unable to determine whether a living organism be a cause or a consequence of the febrile state, of fermentation, or of putrefaction, essential to either process, or only incidental, causal or casual, a phenomenon or an epi-phenomenon. Where those recondite explorers of fever arcana fail, the practical physician, who clearly dare not pursue similar experimental methods, needs no apology for any shortcomings in scientific precision.

The vast number of unsolved problems in other departments of biology afford some extenuation of the still unsettled questions in the pathology of zymotic fevers. When phytopathologists can fairly explain whether the fungus on the potato, or that on the vine, is solely contagious, or say under what combination of causative conditions it is autogenetic, then may they turn and beckon to the more abstruse study to emulate their success.

Meanwhile, by conditionally admitting the truth of both theories, it is possible, by com-

binning them, to adopt an empirical practice of sanitation that would efficiently "cover both issues," according to the views of the ablest teachers, from Louis to Niemeyer, or even the most zealous sanitarians of the present day. In resorting to this compromise of theories for the sake of practical expediency, the people of Melbourne may at once dismiss from consideration one source of contamination much dwelt upon elsewhere as a frequent vehicle of typhoid poison, namely, water supply; and fall back either upon direct contagion from fever patients, or upon manufactured poison from foul drains or decomposing organic matters in a thousand forms of putrescence. From both sources there is no lack of causes to account for the prevalence of typhoid fever in and around Melbourne.

It is almost impossible that the water-pipe can be the vehicle for carrying about the typhous particles. The constant pressure system, and absence of communication with closet service, forbid the supposition. It is true that in early summer the deficient supply of water often causes intermissions of pressure,

but except from the hydrant valves for street watering, there can be no suction of fever-breeding impurities. At one time these valves were placed in the channels; but that dangerous arrangement was discontinued some time ago, and they are now placed out of risk of causing harm. The drainage from the water-closets of the hospital has also been diverted from the street channels. Unless it be arrogantly in some government building, or surreptitiously in some private house, there is no water-closet service to endanger the drinking water.

In now more directly pointing out some sources of typhoid fever, or channels for conveying contagion, a few particulars, brought under the author's personal cognizance, of the surroundings of persons who were not long ago suffering from typhoid fever, shall now be briefly related.

Early in last autumn a family of eight were, one after another, seized with well-defined typhoid fever. The father, a man of middle age, was the first attacked, and died in the beginning of March. I saw him for the first time three days before death, and about the

twentieth day of fever. Two days before his decease, his eldest boy, a lad of seventeen years, was observed to be ill; in his case the fever lasted about five weeks, with severe local complications, very great prostration, and large bed-sores, but he rallied through protracted convalescence to strong health. Three younger children fell ill during the earlier stage of their brother's fever, and were sent to the Alfred Hospital, where in one of them the fever proved fatal. At a later period another child, a girl about ten years of age, was seized after the family had removed to a different house; the fever was in this case severe, but favourable, with recovery. The mother who had fever in early life suffered from prolonged febrile debility, as if the contagion had not been powerful enough to affect her fully, through the protection afforded by her former attack. The detached wooden cottage in which the family lived when they first became affected, stood on a very healthy site near Elsternwick, the soil being a light sandy loam, with dry gravelly subsoil; the cottage was well ventilated for a building of the kind; and previous to the

fever the family all enjoyed excellent health, the children being strong, active, and hearty. After most careful inquiry into the origin of this fever, no history of contagion could be traced, further than the probability that the father, who was the first seized, and the only member of the family who went from home: he, by his work as a coachman having often had occasion to sleep out, may have been so exposed; but as he was moribund on my second visit it was impossible to interrogate him as to whereabouts he had been travelling, or to elicit from him any hint which could be followed up to some probable source of contagion. The water supply of the household was not from the Yan Yean, but from a sunk tank close by the back door of the cottage, and within some fourteen feet of the pit of an old filled-in privy. The tank had been low in water during the end of summer, and some maggots had been seen in it. For this reason the water was not used by any of the children, although the father sometimes on a hot day preferred a drink of it because it was cooler than the water from a neighbour's iron above-

ground tank, which the family had been using during the short supply from their own. When the heavy rain fell at the end of summer, and immediately before the outbreak of the fever, the sunk tank was suddenly filled, and the dry sandy soil around became saturated with moisture; but whether any percolation, or rather diffusion, took place between the tank of water and the old privy soil, or not, it is hard to say. On digging into the old soil the black rotten stuff was horribly offensive. There is an imperfect history of fever having been among the occupants of the same cottage, and therefore in the users of the same old privy, some three years previously, when the old privy was in use. It was at once readily surmised that the first case of fever had been caused by drinking the water of the tank, conjectured to have been polluted by the contiguous old night soil. A close inspection, however, showed this to be hardly possible, the relative positions of the tank and the privy making it unlikely that any contamination had occurred. The tank water stood at a higher level than the buried night soil, and was protected by a

thick clay puddle; although this coating, as well as the inner cement of the brick tank was rent by the roots of a tree. This accident made soakage not impossible. The families living in houses on either side of this sick family were both invaded by the same typhoid fever; in one of them three children had well-marked seizures, but there was no death amongst them.

After reviewing the circumstances attending this severe local outbreak of typhoid fever, the opinion arrived at was that the father had caught contagion on a journey, and had brought fever home. Fever was rife in all directions about that time. It is, at any rate, quite certain that his premonitory symptoms were noticed by his friends and also by himself before the rainfall of autumn, for his first rigor was attributed by him to a wetting in the first shower that fell on the setting in of the rainy season. But the malaise was evident before that, as he had been advised to rest from work on account of his evident weakness, advice that he declined as men sickening for typhoid fever generally do. This must have been during the drought

of February. Even upon the supposition that this fever had been produced by using the water polluted as suggested, it might not have been merely ordinary fæcal impurity, for possibly the soil contained specific fever germs, latent in the contents of the privy, and derived from the fever cases said to have occurred in the cottage three years before. The tank water was not analysed, although a proposal was made to that effect, to determine the amount of animal matter, if any, in it ; because the water was obviously not then in the same condition that it must have been in while the man was drinking it, prior to his illness, so that any subsequent analysis could only have led to a fallacy.

Explain then the first of this marked series of typhoid fever cases however we may there is little doubt that the other consecutive cases of the series prove the spreading of the fever by contagion, which radiated from the father to the immediate family circle and thence to the next door neighbours close by on either side. Before I visited the case the attendants, never dreaming of contagion, and having

received no cautioning on the subject, merely disposed of the patient's dejections upon the ordinary heap at the rear of the yard, or at the foot of the garden, or into the shallow cess-pit of the common privy, allowing them thus to dry in the hot sun, and be wafted about as an impalpable dust in the air which the children playing about must necessarily have been all day long freely inhaling. Of one thing I am positive, that the fever was not set up by effluvia from exposed drains or decomposing or fermenting organic animal or vegetable matters, because nothing of the sort was about the premises. A more orderly cleanly airy dwelling could not possibly be found; so that, if none of the facts help to illustrate the theory of contagion, assuredly they lend no support whatever to any pythogenic hypothesis, unless we adopt the supposition and infer the source of poisoning to have been in the old soil of the filled-in privy. Whatever the explanation, the local outbreak exemplifies how suddenly a strong actively healthy family may be unconsciously smitten by the stealthy typhoid poison, and well illustrates the

importance of medical science determining by what agency so severe a calamity can happen, and how the provident head of a family can shelter his charge against it. At present amid the conflict of divers medical theories, medical practice is absolutely impotent to say either how such an illness comes, or how it can be averted. Every family who wills it may save itself from the inroads of common phthisis, for the ordinary exciting causes of the varied forms of that disease are well known and can easily be appreciated, its victims mostly rushing into danger open-eyed; but no one knows when, and the medical councillor is unable to tell him how, he or his may be unwittingly forced to breathe or swallow with usual food the deadly germs of typhoid fever—a rather pitiful plight for modern medical science. The theme could easily be enlarged upon by similar episodes, with trivial variations of detail, without making the mysterious workings of the agency less inscrutable, or the helpless attitude of practical medicine less inglorious, nor more thoroughly demonstrate the imperative obligation devolving upon the profession

to renew its studies of the subject, so that it may lift itself out of the humiliatingly helpless attitude.

About the time of this family's illness I saw a boy in typhoid fever in Grey-street, East Melbourne. This attack was very marked. The house was one of the best in that elevated locality; airy, clean, dry, and free from foul or obstructed drains or closets, or putrefying, decomposing, or fermenting organic matters of every kind. The family were all in good health up to the time of the fever. At first we were puzzled to say how he could have caught fever. Upon inquiry, it was ascertained that the nurse-girl had been ill for some weeks, and that, although she still went about nearly all the time, taking fever medicine the while, as if suffering only from *la fièvre typhoïde ambulante*, she was nevertheless unfit for domestic work, and was entirely confined to bed for more than a week. To save expense she went to a druggist's for advice and physic, for what she believed to be only a mere ordinary diarrhœa, a not uncommon medical error. It appears that she had some time before her

illness sent a dress to be repaired, and the dressmaker on returning it, apologised for detaining it an unusually long time, over six weeks, relating how her child had at last recovered from a long and severe attack of low fever. When spoken to about the risk of fetching fever into the house with her, she replied, "Oh, there is nothing in that, for the doctor says it is not catching." The girl afterwards noticed that the dress was draggled with mud on the skirt, as if it had been worn, and expressed her anger at the dressmaker's want of care. Here was the mystery of the fever explained by a clear instance of contagion by fomites carried in the dress; possibly some of the alvine discharges having fallen upon it in the small fever chamber. A second younger child took the same fever just as her brother was recovering and had a sharp illness. The other children of the family escaped by having been sent away from home. The mother who nursed both children was herself protected by having had fever in former days, when she, with a sister who died of it, and several others

caught it from a shepherd's child brought up to the station on the Murray, ill with fever from Melbourne, about 1854, at a time when, as well we know, fever was very rife in town. In the fever in this family in Grey-street there was no history of spontaneous origin, but the most obvious descent by communication from person to person. The medical members of the Central Board of Health are of opinion that there is a lack of unmistakable evidence that this nurse-girl was suffering from true typhoid, or that the dressmaker's child had the same species of fever. This objection of theirs I cannot answer, not having myself seen the cases; but by subsequently advertising through *The Argus* for the medical attendant of the dressmaker's child, and instituting other inquiries, I have at length after much trouble found out that one medical man about February last prescribed at his own residence for this woman and her child who was sick at home, and too ill to be brought to him; and I further learn that the woman called in a neighbouring practitioner, who was for several weeks in constant

attendance upon not one child only, but on the poor dressmaker's *three* children who, with their mother, were, by her own confession, kept in bed for three weeks with what was designated "*low fever*." This medical gentleman, a very old and infirm man, was at the time of my learning these additional particulars on a sick bed and too feeble to be interrogated further on the subject. But, had my circular, sent out in April last, been duly responded to, all these facts would have been fully elicited, and thus would have spared us from the Board of Health's captious objections.

Perhaps it may be further demurred that the "*low fever*" was not true typhoid; and as people appear to believe in an endless variety of continued fevers in this city, unique in that particular, one is at a loss to conjecture what other order it could belong to. This alone I can aver that the cases directly descended from them were "unmistakable" typhoid, of a severe type, and the Board of Health ought to be aware that a severe case of any contagious fever may be taken from a mild one; and also "that many cases now returned as

simple continued fever, are in reality slight forms of typhoid, a matter of vital importance to the community, the infectiveness being equally great, in the slighter as in the severer forms."¹ At any rate, if there still remain a doubt as to the contagious source of this fever in the house in Grey-street, there can rest none as to the entire absence from the premises of any pythogenic source of fever.

Again, in March last, I saw, on the day before her death, a young lady living on Hawthorn Hill. In answer to my inquiry, the medical gentleman who was in attendance informed me that there was no eruption on the patient's body; but on examining her the whole surface of the skin was seen to be covered with one of the most copious eruptions of true typhoid spots that either I or any one else ever had an opportunity of witnessing. How they could have been unnoticed is perfectly amazing, and fully justifies the little reliance that is often placed upon what one casually hears of about such occurrences.

¹ *Aust. Med. Jour.*, May 1874, p. 152.

There were neither stagnant pools, foul drains, stopped-up sewers, putrescent cesspools, nor any of the usually accounted pythogenic sources of spontaneous fever poison anywhere about the premises; but the daughter of the family milk-woman had been brought home ill of typhoid fever from a school in town just immediately before this young lady was seized by her fatal illness. There were also several other cases of typhoid fever in the immediate vicinity, all in equally good sanitary surroundings, and all seized simultaneously, but I could not at the time learn whether or not they had been supplied with milk from the dairy of the woman with the sick school-girl. My impression from what I heard is, that they all were so served. To this inference likewise the Central Board of Health demur, alleging that there is no "unmistakable evidence that the milk-woman's daughter had real typhoid fever," and *ergo*, that my patient's fever had not been contracted from that source. To place this point beyond dispute or further cavil, and remove the Board's objection, I waited upon the physician who had attended the case, and he assured

me that, and I shall use his own words, "a more decidedly well-marked case of true typhoid fever could not possibly be seen." "And moreover," this gentleman voluntarily continued, "I have heard that six or seven families supplied with milk from the house all had the same fever." To confirm this, if possible, I next visited the dairy-woman and asked her for a list of her customers, stating the purpose of my request, but she *point blank* declined, saying "all the information she could give would not help me much, as she only served seven or eight families." If, as her physician heard, six or seven families supplied by her had somehow then contracted typhoid fever, and she only served seven or eight families altogether, it follows that nearly every family so served must have had typhoid fever at the same juncture. I have since learned all particulars of three of these families, and am perfectly satisfied that my original conjecture was right. Of course this does not satisfy the exactitude of proof required for demonstration, and it is proper to be extremely circumspect in admitting hearsay or circumstantial evidence

in scientific inquiries; but I apprehend that few people outside the central bureau will entertain any moral doubts about the series of events being somehow connected by the links of a common chain of causation. But moral doubts do not constitute scientific certainty. To attain this, I asked for some authority from the Board to insist upon this milkwoman giving me the required information; but the Board appeared to see as little utility likely to come of the proceedings as the milkwoman herself, and, like the gruff old Abigail, refused my application. The scientific test, therefore, reverts to the region of mere moral conviction.

Late in November last, while in conversation with a medical gentleman upon the cause of typhoid fever, he mentioned that he himself and two of his children had typhoid fever six months previously, that is, in May last; and asked me the question, how they got it? In reply, I suggested some defective sanitary arrangements about his residence, but he assured me of scrupulous care taken by him in those matters. To help him out of his dilemma, I half quizzingly supposed that they must

have got some of the milk that I had reason to fear was so fatal to some of my acquaintances, when he exclaimed, "Stop! your remark recalls the fact that, about ten days before I fell ill, I had been visiting a family at — ill with fever. The daughter was at service in Carlton, and called on me ill for advice. I sent her home to her parents, and next day her father came to ask me to prescribe for her; but fearing the case might prove one of fever, I went, about seven miles out of town, to see her. I attended frequently for some time. Four younger children were afterwards taken by the same fever, and all recovered. In gratitude for my kindness they sent me a large quantity of milk daily, of which both I myself and two children drank freely, and in about eight days we all three were down on a sick bed with well-defined typhoid. Indeed, I was so suddenly attacked and the rash was so full that it was almost like typhus. I was very ill. The two medical friends who had charge of us agreed in pronouncing the fever true typhoid; and, until you mentioned the milk, we have always

wondered how I could have caught it." On hearing this narrative I begged leave to refer to the circumstance, and received his permission with the *proviso* that I should not divulge the milkman's name, "for his customers would all leave him." I promised, but observed that secrecy was not necessary, as the danger was now past; although, if it had been present, that the fact would surely have made publicity all the more obligatory. But the stipulation proves what singular ideas people entertain of medical confidences in regard to contagious fevers. I also visited the dairy and heard from the people themselves the whole history of the fever. The father had an idea that it originated from offensive smells from offal buried about the fields for manure; and felt rather surprised at his forming such a notion, when I reminded him that these bad smells had not produced any fever before his daughter came home to him ill of fever from service at Carlton. I made a second journey out, and endeavoured to persuade him to furnish me with a list of his customers in Carlton and Fitzroy, some seventy or eighty in number, so

that I might trace out whether any of them had suffered from fever simultaneously with the medical man's family. He replied that he would first consult with that medical friend, and leave the list with him; but on inquiring for it afterwards, I was assured that "none of his customers had any fever," and beyond that information was denied further particulars. The families concerned alone know if it be accurate. On one occasion of public panic, while endeavouring to explain to alarmed stockowners how *foot-and-mouth disease* was brought into the country, the importer of the diseased animal, with its attendant *zooglæa*, fervidly defied me to the proof, offering to give one hundred guineas to a public charity should I succeed. I did. As long as there is so much concealment, or false sense of neighbourly duty, on these occasions, so long will dealers continue with impunity to distribute contagion with their wares, and the mysterious sources of fever remain a puzzle to public hygienists.

These fresh details about the dressmaker's children, the new fact of the Hawthorn milk-

woman's child's fever being undoubted typhoid, with the additional fever episode in the family of my medical friend, are not embodied in my report as printed by Government. I wished to amend the report to that extent before it was published, by inserting the later ascertained facts, but was refused permission.

For official reasons the Board of Health wittingly sent an incomplete statement before the public. Otherwise it could not have failed to supply the suppressed data, even where these force it to yield its no longer tenable defence of pythogenesis. To ensure a final revision of my report is the object of this re-publication. Had the few facts been interpolated, the report would have been left unaltered.

In calling on me to fulfil an onerous duty the Board seems to have thought of pecuniary pay, forgetful that such work must either be beyond price or worthless.

Had I been made aware of the intended offer, the task would have been declined, and my studies of the great problems continued on a plan of which it was no part to "propose money as an end, and knowledge a means of

obtaining it ; but, reversing this process, to make knowledge the end, and such money as I possessed the means of obtaining it.”¹ For I did trust that, for voluntary labours in a self-imposed task, there might not be denied even to my humble efforts some small share of the pleasure derived “by men whose sole reward for their labours was the noble excitement of research and the joy attendant on the discovery of natural truth.”

If curiosity seek other excuses for the Board’s refusal to let in a key-stone to my arch of proofs, it will find them in the commentary. Referring there to the Hawthorn series of fever cases, the Board declares that “*the link unmistakably connecting the disease with the dairyman’s family is wanting.*” What can that link be ? Is it direct proof that the dairyman’s child’s fever was undoubted typhoid ? If so, the missing link was “*forged*” by two good and true men, who medically guided the patient through her illness. With that trusty workmanship the doubting Board would neither shackle the new link to my chain, nor alter its own disingenuous bulletin. It had already

¹ Prof. Tyndall, *On Light*, p. 213.

formed an opinion of the cause of typhoid fever, and "*hoped*" my inquiries would trend towards the foregone conclusion. In avowing "*hopes*" that these "*inquiries would find conditions common to different localities that would prove a common cause of the disease ;*" and afterwards, in finding nothing in nature reflecting its own mind on the subject, it expresses "*regret that its hopes that this inquiry would have led to a satisfactory elucidation of the important point now in question have not been fulfilled.*" Hence its chagrin, and a rejection of hostile facts found after long search, delay, and trouble. The Board required me to work a miracle, and bring sanitary science down to its way of thinking. It wanted a demonstration of what has no being—a cause of spontaneously-formed fever-poison in universal festering groups of "*insanitary conditions that lower the health of a locality and the vital power of its inhabitants.*" In failure, it is no longer armed with quasi-scientific warranty for harrying the towns with fumes of tar and brimstone. If not content with the humble lot of master scavenger, it can no longer usurp the showy office of general

fumigator. Carbolized pitch was its incense ; and like some other incense burners it hates the truth.

With conservators of public health so unenlightened upon the modes of fever, it is hardly wonderful to hear illiterate cowmen or lettered editors dogmatise over obsolete fancies about specific virus "*naturally developing itself most actively in bodies with vital energy impaired by exposure to miasms,*" and so forth. They think they repeat the admitted axioms of science, when they are only re-asserting common-places that recent science has disproved. For if there be any one truth respecting typhoid fever now quite established, it is, that lowered vital power is not even a common, far less is it a necessary antecedent, or concomitant of fever ; because, in actual life, typhoid fever is daily found far more liable to take the strong and healthy, living in good conditions, rather than the ill-conditioned and delicate, living in bad conditions. If people cannot understand how this oft-repeated truth should be, so opposite from what might "naturally" be expected of a fever, they may be helped in their difficulty by an

analogy. Thus, every mother who has had a baby vaccinated knows that she watched and waited for a time when it was in good health; for she knew that any disturbance of its system would interfere with the natural action of the specific vaccine virus with which she was about to inoculate its pure blood. Now, what happens in vaccine inoculation, happens on precisely the same principle in typhoid inoculation:—A minute spheroidal particle of living albumenoid matter, called by some a *microzyme*, because it is a living organism of specific form, possessing as definite living properties as a pea, is introduced into the *liquor sanguinis*, where it finds a suitable soil in which to grow and multiply, just as any seed sown in soil multiplies and reproduces its kind. In one case the little particle enters the circulation through the skin of the arm, in the other case mostly through the inner skin, or mucous lining of the bowels. But, in both cases, when once within the circulation, the *modus operandi* is the same; each alike requires prime, pure, healthy blood to operate upon to produce its appropriate and characteristic effects, just as

a pea needs good rich if possible virgin soil ; for in vitally impoverished ground peas will not grow, nor *microzymes* in poor *liquor sanguinis*.

Perhaps it would be too much to ask an editor to comprehend this, so contrary to what he would expect "naturally;" but he may come to understand how it is, some day; just as of old, people who "*naturally*" saw the sun go round the earth, were taught by *unnatural* science that their belief was an illusion, as the earth it was that turned around the sun. It would be well then for editors to stick to their morals and politics and studies of sociology, uncertain sciences, sufficiently far behind to require all their ingenuity, which they could better devote if they left off dabbling in medical problems, until at least they had by some study acquired an acquaintance with their rudiments. Sufficient as they usually are, they still cannot know everything. "'Delivering brawling judgments unashamed on all things all day long,' has done much to deaden the small stock of individuality in public verdicts, and to make vulgar ways of looking at things and vulgar ways of speaking of them stronger and stronger, by

formulating and repeating and stereotyping them incessantly from morning until afternoon, and from year's end to year's end." "We are," says Professor Bain, "all blockheads in something; some of us fail in mechanical aptitude, some in music, some in language, some in science. * * * There must be in each case a deficiency of cerebral substance for that class of connexions."¹ Nature is economical in her distribution of cerebral substance, and man should follow her example in the use of it.²

This study of the mode of reproduction of the living particles of specific morbid poisons, now engaging so many minds, will be greatly aided by the researches of Dr. William Osler on organisms occurring in the *Liquor Sanguinis*

¹ *Mind and Body*, 1873, p. 93.

² Some time before the current *diademic* of measles, I called at the office of a leading daily newspaper, to advise it to warn the public of the near approach of the contagion. The proprietor agreed, but the editor "could not see the use, because everybody must have the measles." Many people who have not noted the mode of transport, think the outbreak an instance of pandemic wave; but in plain truth the contagion was a fresh importation, free of duty, and not challenged at the Custom House.

in health.¹ Until the actual normal constituents of the blood be ascertained by investigations of that description, the abnormal states cannot be determined. They are among the preliminary experimental studies that will eventually enable some one to solve the problem of zymotic fever.

In a city like Melbourne it is not easy to trace through the comings and goings of people when and where they have been exposed to contagion. In many cases I would have had no difficulty in forming a conjecture of pythogenic infection from the foul surroundings so familiar among residents in the crowded parts of the city, and by no means rare in the sparsely-peopled outskirts. But, having traced very direct sources of carried contagion in several instances, disposes me to look to that as the main mode of propagation, if not really the only method of perpetuating typhoid fever. We have seen that contagious *foci* are never absent at any time of year, but are perennial; although at certain seasons

¹ *Proceedings of the Royal Society*, June 1874, p. 391.

more numerous. The specific discharges from the diseased bowels of a solitary case, if thrown out on the dry ground, desiccated and dispersed through the air, could simultaneously affect many people living in widely-distant localities. While a heavy shower of rain, by flushing channels and washing dirt heaps, would clear away the poison and lessen fever, or even make it almost disappear for a time, sufficiently explaining the low fever-rate of winter and high rate of autumn, without resorting to hypothetical fermentations fomented by heat and moisture.¹ Certainly, in a city with rough and primitive surface drains, the modes of disseminating the poison must differ from the manner of diffusion in other cities having complex underground drainage, and water-closets supplied by the same water-service that carries drinking water, bringing closets and fountains into direct connection.

¹ "That something more than heat and moisture are necessary for the production of enteric fever is obvious from the exemption by some young regiments at stations where both are experienced in abundance." What can that "something" be, if not a specific reproduced *contagion*?—*Lancet*, Oct. 31st, 1874.

In this still rustic city,² typhoid germs fly abroad exactly as they are scattered in rude hamlets and rural places, where, possibly, the cessation of vegetation in hot dry autumnal weather conserves the animal matter that operates from sick to healthy people by intermediate poisoning of the ground, and which would speedily be decomposed and absorbed by active vegetable growth. Doubtless many typhoid fevers ending fatally in wet April and May have extended on from the dry March or February, the unobserved period of their inception.

Acting on the postulate that typhoid or enteric fever is alone kept alive, so to speak, by the

² Melbourne has been poetically inspired to polish her manners and grow the centre of fashion and emporium of commerce—the Cynosure of neighbouring eyes, and a merchant of the people of many isles—two noble cities rolled into one—Athena's efforts can no further go: to make a third she joins the former two. If thus one raw *Bathybius*-formative mass of social protoplasm absorbs two ripe civilizations, she imbibes them also medically. Her peculiar blessings will not come free from peculiar woes, and she will have to conjoin the genius of Chomel with the numbers of Louis; the exhaustive Murchison with the logical Jenner; for, whatever our city may possess of the mixed capabilities of Paris and London, so is she like them both, a hot-bed of typhoid.

specific poison which is cast off from the glands of the bowels of those affected, and which, being liberated on exposure to the air, is inhaled or swallowed with the food on which it may have settled, it will be obvious that the only safe or necessary instruction to give to the attendants on the sick is to destroy this poison in the stools the instant these pass from the body. If thrown into a privy or a drain, the privy or drain surely becomes, *in re*, the poison, "only the continuation of the diseased intestine," for this is really the relation of sewers to typhoid fever, on the contagionist theory. The great practical importance of this fact I cannot urge too strongly, because of having lately seen people placed in imminent or actual danger of contagion by having been assured by medical attendants that typhoid fever was not contagious.

There may perhaps, by this postulate, be a practice of sanitation that will efficiently cover both issues; but if, according to the pythogenic theory, we look chiefly to the putrescencies of foul drains containing fermenting fæcal or other decomposing organic matters, poisoning air,

food, or water, as the main source of typhoid fever, and do not regard the alvine discharges from fever patients as alone specially poisonous, it is clear that we may purify and sweeten drains and remove decaying substances, yet leave untouched the subtle poison. On the other hand, if the discharges from the bowels of fever patients be acknowledged as the only source of specific typhoid virus, it becomes equally clear that they may contaminate air, food, or water, without ever reaching a drain or cess-pit; for the virus may attach itself to soiled bed or body clothes, be dried thereon, and afterwards be carried about or dispersed, as fomites, in many unsuspected ways, unless it be at once destroyed on passing from the patient's body.

Although foul drains and cloacæ may not, therefore, be the laboratories for generating specific fever germs, they nevertheless are detrimental to health in various other ways; but this pernicious influence has already been shown to form a subject for an entirely different inquiry; one quite as important to public health as the causes of fever.

Assuming the action of specific morbid poisons as the true mode of propagation in contagious fevers, the recognition of the natural law suggests a revision of nosology. In the nomenclature of medical writings, causation is seldom considered, but it is recognised in the terms used in the system of classification adopted by the Registrar-General. In that system the class zymotics is too comprehensive, embracing as it does the orders miasmatic, enthetic, dietic, and parasitic diseases; and the effect of this is seen in a comparison of the extent of the so-called zymotic diseases in this colony and England. Thus, hydatids, so common here, are not zymotic, nor is alcoholism nor want of breast-milk, nor lues venerea; while some only of the miasmatic affections are zymotic. The subject certainly requires re-adjusting.

At the risk of adding to fever nomenclature, the terminology would be rendered more precise by introducing the new term *diademic*, to signify an order of true zymotic fevers; and I have ventured to adopt that new term to signify a fever that passes from person to person

through a people, as small-pox, measles, and typhoid fever do certainly pass.

Perhaps it may not be deemed presumptive to propose this slight innovation, which, if adopted, would at least supply the desideratum of a term definitely expressive of a theory of fever causation; for certainly the diseases of the diademic order are the only true zymotics, in the sense of a specific morbid action, analogous to fermentation, excited in the blood once for all. The poison of lues venerea is not a true zymotic; once introduced into the system, it is never eradicated as a true zymotic poison is removed. Men may and do beget healthy issue after suffering from a true zymotic, after lues, never.

As further proof of the growing belief in typhoid contagion among members of the profession, it may be mentioned that in recent local epidemics of typhoid fever thoroughly traced out to their sources, in no instance was the poison followed to a pythogenic origin, but, in all instances, to a previous case. In the last of the series this was especially so, when Dr. Murchison was

instrumental in demonstrating that the contagion-carrying milk brought to his own family came from the dairy where a man in typhoid fever contaminated the water that in its turn poisoned the milk. In a series of articles in the *Medical Times*, of July last, on the "Nature and Signs of Typhoid Fever," the editor of that journal advocates the view that not sewer gas *per se*, but the gas *plus* a specific germ, causes fever. The same view is taken by the editors of the *British Medical Journal*, and *The Practitioner*, in reviews of the evidence on a recent outbreak of typhoid fever in Caius College, Cambridge, where Drs. Paget, Bradbury, and Buchanan found the sewer-emanation actually contaminated with the specific typhoid matter. In truth, the further the investigation is carried, the more it tends to culminate in a demonstration of this theory of specific poison; and I venture to predict that, although strange just now, it will ere long become the universally-held opinion among the profession in this colony. It is barely thirty years since typhoid fever was differentiated from typhus; indeed scarce fifteen since

Jenner finally decided the point; and it is not yet much over thirty years since the notion first dawned on the professional mind about a specific fever germ, concocted not in sewers, but in pustules in the coats of the bowels, just as fever germs are formed in pustules in the skin in small-pox; and it takes at least a generation to drill a truth like that into the medical mind, however ready it is to run after frivolous novelties. "In the positive endeavour to realise an opinion, to convert a theory into practice, it may be, and very often is, highly expedient to defer to the prejudices of the majority, to move very slowly, to bow to the conditions of the *status quo*, to practise the very utmost sobriety, self-restraint, and conciliation."¹ The need of this prudential action has been amply shown in the history of the growth of medical opinion of the contagiousness of typhoid fever. Neither the lay nor the medical mind in this colony has undergone the renewal change; but perhaps both now are touched by the mental catalytic.

¹ *On Compromise*, by John Morley, 1874.

The Chairman of the newly inaugurated Metropolitan Board of Works, who ought to be an authority upon sanitary laws, recently denied the presence here of typhoid fever, and challenged me to show him an example. Accordingly, I took him to the Hospital, and pointed out a typical case that had been, on the day previous, brought under my own notice by the courtesy of the honorary physician who had charge of it.

But the chairman demurred to look upon it as a typical case of typhoid fever, because it did not resemble what he had seen of fever in England, where the patient, lying prostrate in low muttering delirium, with dusky countenance, dull inanimate eye, sordes-coated teeth, dry brown tremulous tongue, livid speckled body, and retracted belly, contrasted so markedly, before even the casual observer, from the clear complexion, bright nacreous eye, sardonic smile, rosey-pimpled body, tumid meteoric abdomen, clean teeth, and generally impatient aspect of the case before him. But, what he looked upon was a true case of rosey TYPHOID fever; and that which his memory

retained so vivid a picture of had been a case of petechial TYPHUS fever. As well, therefore, might the Chairman of that newly constituted Board, which is in future to be entrusted with the guardianship of our public health, say of Gerhard, or of Louis, that, because they rarely saw *typhus* in Philadelphia or Paris, they wrote foolishly of *typhoid* fever; as to presume that, forsooth, because we have no typhus, neither have we typhoid in our midst. All the more significance is due to this episode, because of the official influence of my new protégé in the study of typhoid fever. He had an official medical health officer for his tutor, who now may be persuaded to become his pupil, and at the feet of Gamaliel study the difference between typhus and the equally fatal typhoid.

The newly formed Metropolitan Board, tainted of adulation, was organized upon the spur of the moment, a transient reflex of a genial whim; and must either conflict with or abrogate other already existing Boards, that are charged with conservancy of public health, so that if it be required they must have failed to do their work. It is less new machinery

that is wanted, than men more vigilant, sterling and earnest, to watch the tread of fevers, and step out quick for their extinction.

On the day before our visit, I was privileged to witness at the Hospital an autopsy of a case of typhoid, where the specific ulcers were well marked ; but I saw the nozzle of the water tap inserted into the diseased gut, and its contents washed into the street channel, to be sent no one knew whither, to cause, perchance, some fresh cases of typhoid, of origin unseen, and, therefore, inferentially spontaneous. For there was in that bit of specially diseased intestine enough of typhous virus to inoculate half the fever-susceptible subjects in the community. And this is how the diseased gut and the dirty gutter are made joint conduits for conveying typhoid fever.

As it is with the causation of typhoid fever, so has it been with the origin of diphtheria. An inane persistency has thrust forward the prerogative of ditches, and ignored the natural liberty of contagion. In the late Report of the Central Board of Health, on the terrible decimation at Yea, the word *contagion* never

appears. In a rural valley, clad with a cluster of cabins, the dwellings of two hundred and twenty souls, there were within a few months, seventy-five cases of diphtheria, and twenty-six deaths. The Board of Health not only ignored contagion, but with a mendacity that smothers truth, it silences the report of its own Inspector. This allegation can only be verified by comparing its Report, page 9, with the following official history of the entrance of diphtheria into Yea, which, honestly, should have been incorporated with the Board's official report. The public Press often expresses wonder why medicine fails to bring science to bear upon the elucidation of the causes of the scourge; but while that science is crippled by moral cowardice, or by unveracious "*reports*," how can it clear away the mystery? The only practitioner residing within a radius of eighty miles of Swan Hill, declares that, during his long experience of seventeen years, not a single case of diphtheria or of scarlatina has occurred that was not imported. And Yea would have indefinitely remained equally free had no contagion germs been brought to it.

Memo. in reference to the causes of the recent outbreaks of diphtheria at Yea.

“ The following carefully ascertained facts most certainly tend to show that the disease was imported into that township by contagion : Mrs. McNamara's infant, six months old, was the first case. The mother and child on last New Year's Day, left Yea, travelling by coach to visit some friends at Molesworth, about twenty or twenty-five miles distant, and in the same coach were also travelling another woman and baby, *en route* from Ballarat to Aléxandra. The latter baby was only just recovering from diphtheria. On the following day Mrs. McNamara and her child returned to Yea, and the same evening the child was taken ill with diphtheria, and died a few days after. Mrs. McNamara, herself, was the next affected, but she easily recovered. The Jackson family, near neighbours, suffered next ; and then the McKenzie family suffered very severely, seven children dying in quick succession. These children were frequent visitors at McNamara's house. Several other cases occurred, and there were many other fatal cases in this first out-

break. Near about the same space of time intermitted between the first and second, and the second and third outbreaks; but from the first the disease would seem to have fastened on the State School, probably in the closets, for in the second and third outbreaks, the first, and most of the other cases, certainly emanated from the school. As regards the cases (five fatal) in Mrs. Kerr's family, residing about five miles from Yea, it is well known that the tutor to Mrs. Kerr's children had been visiting Mr. Oliver's child and the nurse (both died) at the time they were ill with diphtheria. It is also worthy of note, that Mr. Webster, the President of the Shire, asserts his conviction that after visiting this same nurse, the day previous to her death, he carried home, three miles distant, the contagion to his children, as during the whole time of these outbreaks no other member of his family visited the township." Those are contagion facts—the following, pythogenic guesses:

In forwarding "the accompanying Notes of some cases of typhoid fever which occurred in the Sunbury Industrial School in 1873 and 1874," the writer remarks:—

"I think the four cases which I have detailed as genuine typhoid point to a pythogenic origin, rather than to contagion.

"The advocates of contagion as the only source of this fever, may however lay stress on two circumstances; (1st), that while the first case here recorded as true typhoid occurred in April 1873, a previous case of 'fever' had occurred in the end of 1872, and that it is only necessary to assume that this previous case was typhoid, and thus the missing link is supplied.

"In the 2nd place, it may be said, that while well-marked cases of typhoid are noted—mild, 'abortive' cases on the other hand, are lost sight of, or set down as less important ailments, such as gastralgia, or headache; and that in this way the contagion is kept up, although the cases recorded as genuine typhoid are apparently far between."

NOTES OF SOME CASES OF TYPHOID FEVER
WHICH OCCURRED IN THE SUNBURY INDUS-
TRIAL SCHOOL, IN 1873 AND 1874.

"The first case of genuine typhoid fever which occurred in the school, so far as I am aware, was that of the head teacher.¹

"He came to Sunbury on the 5th of April, 1873, direct from the Scotch College, Melbourne, where he boarded as one of the resident teachers in the College. He states that the College was very healthy, and that he was himself then in good health.

"He gives the following account of the commencement of his sickness :—

"About ten days after coming to Sunbury, he went out shooting one Saturday, and got wet feet. On coming home he remained sometime in the township without

¹ I find two cases of typhoid fever recorded so far back as 1866.

changing the wet covering of his feet, and from that time he felt unwell—having headache, and not feeling inclined to eat.

“Although out of sorts, he kept on duty till about the beginning of May, when I saw him. He was only a few days (perhaps 2 or 3) under my care, when a friend came and removed him to Melbourne, where he was attended by two doctors, who regarded it as a bad case of typhoid fever.

“He does not know of any source of contagion, and regarded his getting wet as the cause of his sickness, until the origin of this kind of fever was explained to him after his convalescence. Even now, and with the knowledge of such explanation, he cannot trace its origin to contagion or infection from any previous case.

“On coming to Sunbury, he found his quarters very dirty. Very near the back door was a surface drain, which terminated just nearly opposite the house. The stagnant sewerage soaked into the ground; and I now learn on inquiry, that urine used to be thrown into this exposed drain, or on the ground near it. The water-closet arrangements seem to have been equally bad. The boys who emptied the closet, state, that the contents were thrown out only a few yards from the house.¹ The earth-closet is generally used in the school, but the closet attached to the quarters in question had not this arrangement, although the boys mention that earth was at times, but not uniformly, put into the bottom of the receiving vessel when emptied. The house had been about a month unoccupied when the patient entered it.

“Rain-water from an iron tank supplied the house; and although the patient noticed some rubbish at the bottom of

¹ Like Mrs. MacClarty, in *The Cottagers of Glenburnie*, they “cou’dna be fash’d to gang sae far wi’ a’ the slaistrey.”

the tank, he does not remember the water tasting or smelling badly. The other source of water supply to the school is from the reservoir. I shall have occasion to refer to this water further on; but here I may mention that the patient is not sure whether he ever drank or used the reservoir water previous to his illness.

“At the time of this case of sickness, and for years before, the contents of the earth-closets of the Institution were carted away and deposited within a few hundred yards of the buildings. An offensive smell was felt from these deposits by the patient when he returned convalescent from Melbourne. The unpleasant smell was no doubt there before he took ill, but he did not pay that attention to it which he afterwards did, when he learnt the probable pythogenic origin of this kind of fever. There were others also who felt the offensive effluvia from the decaying fæcal matter of the closets. It was subsequently carted away.¹

“This gentleman returned convalescent from Melbourne on the 16th of June, 1873.

“The next case of typhoid fever which occurred in the school was that of a boy, who was admitted into the hospital on the 9th November, 1873.

“He states the following circumstances connected with the commencement of his sickness:—‘I put my mouth to the tap in the bathroom one day to drink, and I felt that I swallowed something which had a bad taste. I felt sick from the time that I took this drink, which was at mid-day. About four o’clock in the afternoon of the same day, after school came out, I went to the water-closet and vomited. Looseness of the bowels also came on. I was quite well before this. After being unwell about a week I went into the hospital.’

“I have no doubt about this being a genuine case of

¹ But not till after the two following cases occurred.

typhoid fever. If I remember aright, in my report of the case to the Chief Medical Officer, I mentioned that the characteristic rose-coloured spots were present. This could be determined on referring to the report. The attendant in the hospital tells me that she remembers distinctly about me finding the rose-spots on the boy. I am quite satisfied about my diagnosis of the case, that it was true typhoid.

"I have no reason to doubt the boy's statement about drinking something nasty in the water. He mentioned this same circumstance at the time of his illness; and, acting on the information, I gave instructions that the children were to drink out of vessels, and not put their mouths to the taps. The boy is now about eleven years of age, and of fair average intelligence.

"When the reservoir (from which the tap-water was supplied) was examined, somewhere about five dead cats were found in it, some of them in advanced decomposition.¹

"One fact, however, deserves notice here, as I am uncertain whether it was alluded to in my former Report. Some months before this case of typhoid fever occurred, the bathrooms and water-closets of two of the wards were accidentally burnt down. The closets were burnt on 22nd June, 1873, and this case occurred in November of the same year. It is not unlikely that spots of decaying fæcal matter were more abundant about the school at that time. Many months elapsed before the closets were renewed.

"In mentioning first the boy's statement about the water-drinking, and then in connection with the same case referring to decaying animal matter scattered about as fæcal

¹ About two months before this fever case occurred, some of the wardsmen suffered from diarrhœa, which they attributed to the reservoir water.

deposit, it may appear that I am endeavouring to make out too much, viz. two different sources as originating this case of enteric fever. My object is to state the facts, however varied, and allow each one to supply the inferences, or form a theory.

“The 3rd case is that of another boy, who states: ‘I used to feel thirsty in the morning, and went and drank from the tap in the bathroom. I was told by somebody that the lad in the hospital got sick from drinking the tap water. I then left off. In a few days after this my mouth got sore and my lips blue, and I had a headache. In four or five days after I went into the hospital.’

“The rose-coloured spots were also present in this case, and there can be no doubt about it being typhoid fever. He was admitted to hospital on the 15th December, 1873, thirty-six days after the other. The two boys were not inmates of the same ward, but as regards hygienic conditions the circumstances were very much the same to both. The deposit from the earth-closets had not been removed when these two cases came into the hospital.

“The stools in both cases were disinfected with carbolic acid, at least for part of the time. By part of the time, I mean during the worst part of the fever; for probably about the commencement, and after the crisis, this precaution might not have been adopted.

“I also gave instructions that the stools were not to be thrown into the water-closet, but taken away and buried. I now learn, however, that covering the stools with earth was not always carefully carried out. This work was done by one of the boys in the hospital, and it seems that the stools were taken away with the carbolic acid and thrown on the ground without being always covered with earth.

“Before passing to the next case of undoubted typhoid, I should note a case which occurred in the beginning of the present year, 1874. It is entered in the hospital

register as *Febricula*, and admitted 2nd February, 1874, and discharged 12th March, 1874. It might possibly be a case of mild typhoid, but it is evident that the leading symptoms, *e.g.* diarrhœa and rose spots, were absent, otherwise I could scarcely have failed to notice them.

“The next and fourth case of genuine typhoid occurred in the person of one of the farm-labourers. He went to the Melbourne Hospital on 11th May 1874. He is a married man and lives with his family at the kitchen garden, about three-quarters of a mile from the school buildings. He states as follows:—‘Before I took sick, I used to have wet feet working in the garden which supplies the school with vegetables. I think I also got harm from running after the cattle, being warm and then getting cold. The manure from the piggeries at the farm, and the contents of the earth-closets, used to be brought down and spread on the garden ground. The smell was very offensive. We use, in the house, the Creek (Jackson’s) water, and also rain-water received into a tank from the roof. The tanks had been cleaned out about the beginning of the present year, 1874, when a dead mouse, dead worms, &c., were found in the tanks. I am uncertain whether there was anything wrong or impure about the water when I took ill. I was away at Kyneton for three days in 1873, at Christmas time; but since then I have never been a night from home. I am not aware of getting the fever from any source whatever, *i.e.* as regards contagion or infection from any one else.’ He was admitted to the hospital (Melbourne) and treated for typhoid fever. One of his children also had symptoms of mild typhoid at the same time. His house at the kitchen garden is situated on a slope, and in wet weather the rain runs below the house, leaving it damp.

“The next case was not so well pronounced. It was that of a boy, who was admitted 8th August, 1874. His

case stands in the Medical Register as follows:—‘fever, probably mild typhoid.’ There was slight diarrhoea, but no distinct appearance of rose spots. This case was very mild compared with the two former.

“A boy was admitted 21st September, 1874. At one time I thought this was a case of typhoid fever; but my opinion is now that it was rather a case of scrofulous inflammation of the brain, with phthisis pulmonalis et abdominalis.¹

“A boy, admitted 22nd September, 1874, entered in register as ‘simple fever, or febricula.’ In this case there was a crop of *vesicles*² about the size of pinheads. This eruption had no resemblance to the rose spots of typhoid. I did not regard it as typhoid fever at the time. I am unable to say whether it was ‘miliary fever,’ or not.”

“NOTE.—I am not aware of any case of typhoid fever being in the township or neighbourhood when these cases occurred in the school. I have mentioned a case which is entered merely as “fever,” on the 21st December, 1872.

In these “*notes*” the following points merit attention:—

The first case of the series occurred more than a year and a half ago, and the “*notes*” are all from memory. The clinical thermometer does not appear to have been employed in the diagnosis; not even a self-registering one, to save trouble, and as far as these cases go Wunderlich need not have

¹ This boy died 2nd October, 1874.

² *Vesicles*, and not pimples, as I punctured one or two; a somewhat transparent liquid issued.

written. It is uncertain what share the drinking water, what exposed fæcal matter, and what the other insanitary conditions had in the attributed list of predisposing or exciting causes. The same causes produced like results in only two out of many boys exposed to them. The number of boys attending the school is not given. The boys were all in the daily custom of drinking the foul tank water direct from the tap, but only two of them were ill:—This is unlike the history of drinking typhoid-polluted water, which invariably poisons many victims all at once. The “directions not to drink direct from the tap, but to use vessels” would not, even if carefully obeyed, neutralise or intercept the poison; the direction was inadequate as a safeguard, if the supposed danger really existed. If these cases were *bona fide* cases of typhoid fever of pythogenic origin, derived either from foul tank water, sewer leakage, or exposed excreta scattered on the ground, the wonder is why so few of those exposed to the influence were made ill. Even admitting the *febricula* cases to have been abortive typhoid, the

difficulty is not overcome. The stools of the sick were disinfected by chemicals; which would avail nothing unless they were especially contagious. Otherwise disinfectants were applied to the effect, not the cause. The first case of the series began ten days after the arrival of the subject at the school. This tallies with the malarial theory, which affirms the greater susceptibility to miasms of strong and healthy new-comers, than inured and enfeebled residents; but it differs from the other anti-contagionist theory, that vital stamina being lowered by foul air, lessens the power of the inhabitants to resist fever. Lowered vitality cannot both predispose to and protect from fever. Staid old acclimated burghers jog along scathless, wallowing in mire, until an infusion of fresh blood starts the latent fever poison into lethal activity. The new arrival need fetch no contagion incubating in his veins, to be the first to show the outbreak; yet it is the most elementary fact in fever etiology that specific virus travels *incognito* with hosts unaware of the company. These two main parts of the pythogenic formula—the

greater liability of good and ill health alike, are opposed to, but do not cancel each other ; for they twist as elastic as the slimy python itself, gliding through every argument, and always eluding the grip of logic.

Altogether the "*notes*" do not greatly elucidate the pythogenic hypothesis.

The non-professional readers, to whom my report was at first officially addressed, may not appreciate the points here raised, but experts know that the tests are valid. The diagnosis may be accepted as accurate, although the recorder impressively anticipates a doubt that it might be taken *cum grano salis*, from being furbished up from half-forgotten memories to disentangle the involutions of an after-thought. The cases are evidently raked out of obscurity to enable the Board of Health to confute the arguments for contagion. As such they were declared to be wonderfully important, as affording "unmistakable proof of the spontaneous origin of typhoid fever without contagion," and pressed upon my unconverted attention. Hence their insertion here, irrespective of any intrinsic merit, of the

degree of which the reader must judge. It is fair to let the other side be heard. Besides, if, as Bacon teaches, "negative instances be of most use in philosophy;" and if these "*notes*" be in the estimation of the reader only pythogenic negations, they become positive proofs for the doctrine of contagion.

The report of one of the fatal cases was referred to myself for an opinion of its nature. The details of it denoted *tubercular meningitis* of the form so apt to simulate typhoid fever. It was another instance of the cognate character of febrile disease, with lesion of the adenoid or lymphatic glandular tissue. This adenoid tissue is diffused over the surface of the cerebral hemispheres, and is anatomically allied to the structures before alluded to at page 114, as existing under the submucous surfaces of the ileum and bronchi. This community of lymphatic glandular tissue, in these different parts of the body, would therefore range those almost allied febrile disorders under what may be deemed an extension of Bichat's grand generalisation of the anatomical basis of diseases.

Every objection taken to the foregoing histories from Sunbury, applies with redoubled force to the following meagre account of:—

TWO CASES OF TYPHOID FEVER OCCURRING
IN THE YARRA BEND LUNATIC ASYLUM.

“*Case 1st.*—E. M., female patient in the lower division of the Yarra Bend Asylum, was, on the 1st of June 1874, sent from G ward to the hospital, suffering from diarrhœa accompanied with unusual prostration and debility. After admission the diarrhœa continued, the motions were of a light yellow or pea-soup colour, pulse small, 110 per minute, skin dry and hot, tongue red, dry and glazed, abdomen hard with some pain on pressure in the cæcal region.

“This patient, though not previously robust, was not markedly delicate. She had not been subject to attacks of diarrhœa.

“The character of the motions remained the same, except that from time to time a little blood was observable in them. The pain on pressure still continued, the pulse increased to 120, and subsequently to 130, and was very weak, and two days before death delirium set in. Death took place on the 8th of June, apparently from exhaustion. No eruption appeared at any stage of the fever.

“At the post-mortem examination, which was made by Dr. —, Peyer's glands were found in a state of ulceration.

“*Case 2nd.*—W. J. J., male, aged 46, a patient in the upper division of the Yarra Bend Asylum, was seized on the 7th of July, 1874, with slight diarrhœa, and complained of lassitude and chills. On the following day he was found in what seemed one of his usual apoplectiform attacks, unconscious, breathing somewhat hurried, pulse quick and

soft, and with retention of urine. On the 9th he was sent from L ward to the hospital.

“Very little change was observed in him for four days. The diarrhœa continued, the motions being sometimes maizena-coloured, at other times dark brown, but always loose and frequent. The urine, which was scanty and acid, had to be removed by catheter, and consciousness never returned. The pulse increased to over 120 and was very weak, and the temperature at night was very high. On the 12th of July he showed signs of great weakness, and died on the 15th. No blood was observed in the stools, nor were there any spots seen on the body during life.

“The post-mortem appearances showed ulceration of Peyer’s glands. The body was seen by writer as well as by the attendants immediately after death, but no rose-coloured spots were observed. On proceeding to make the post-mortem examination, Dr. — at once called writer’s attention to the chest and upper portion of the abdomen, which had well-marked rose-coloured spots on them.

“*Remarks.*—The question that first arises out of the two cases above related is, “Were they cases of true typhoid?” About the former case, which was under the charge of Dr. —, and with the particulars of which he has kindly furnished writer, the writer thinks there can be no reasonable doubt. About the latter, it may be said that it was impossible to learn after the second day the sensations of the patient, as he had become unconscious. All the symptoms moreover were masked by the apoplectiform attack, and attention was directed to it, and not to the typhoid symptoms. This accounts too for the possibility of the rose-coloured spots not having been observed during life. That they were the usual spots of typhoid, and not petechiæ of any kind, neither Dr. —, who has made post-mortems here for so many years, nor the writer, had any doubt. It is far more

probable that the spots were present during the last day or two of life, and were not observed, than that they should make their appearance after death.¹

“The question as to the mode of origination of typhoid in the Asylum is to writer puzzling. About two and a half years ago, a case with some of the symptoms of typhoid occurred in the upper division; but for eight or ten years no other case has been observed in the whole of the Asylum. The fæcal matter of the Asylum is disinfected and removed regularly once a day, and conveyed to a considerable distance from the wards and buried. The origination of typhoid *de novo* therefore seems impossible.

“On the other hand, neither of the patients affected were going without the Asylum inclosures, no strangers were making use of the same closets, the drinking water is the Yan-Yean, and the milk is from the cows on the Asylum grounds.”

Besides many other defects in the above “convincing statement,” there is the fatal error of the “dead-spots,” that makes the narrative scientifically worthless. That any one should now-a-day trust for a diagnosis of typhoid fever to the appearance of rose spots upon the dead body, or argue about the period when they came there, affords lamentable proof of the need there is of fresh studies of this subject.

¹ “The spots peculiar to typhoid fever do not remain visible on the dead body.” Sir Thomas Watson, *Lect. on the Principles and Practice of Physic*, 1871, p. 882.

“They are never observed on the dead body.” Dr. Murchison, *The Continued Fevers*, 2nd ed. 1873, p. 510.

Let it not then be said that nothing new has come out of this inquiry ; for have not men seen *both* typhoid *and* enteric fevers ? gastric fevers not typhoid ? spots after death ? spots big as a shilling ? and fever caught by drinking cold infusion of a macerated mouse ? Spots upon the dead body mean somatic life after animal death !

After such solecisms it is no longer wonderful why the answers have been fewer and less exact than might have been expected. Of one hundred and sixty circulars sent to the various medical men, only forty-eight were answered, while such replies as were received were mostly categorically "yes" or "no," and many of them were anonymous, so that the correspondence could not be further followed up. Such as did contain details were carefully dealt with. But truly it may be said that there has really not been in this matter the slightest co-operation on the part of the members of the profession. In only one solitary instance was my request to be allowed an opportunity to visit the fever localities acceded to, and even my personal solicitations on the point were frequently declined. In fact, I contemplated from

this cause relinquishing the inquiry altogether, but finally resolved to proceed with it under all disadvantages. The reply of one gentleman in extensive practice in this city, whose name has often been seen in newspaper communications warning people about the danger to which they were exposed by the neglect of public sanitary work, will afford a good example of the reasons assigned for declining to supply information. He apologised for his not answering the circular sent to him, by observing that "such reports as the present usually report upon everything but the treatment;" to which might have been well replied, that they are not meant to enter into clinical histories, or details of therapeutic treatment, but are solely devoted to questions of causation, propagation, and prevention; and that, if they could successfully grapple with those intricate problems, they would soon teach us how to banish the fever, and render all knowledge of its medical treatment superfluous.

With equal propriety the same objection might be raised against the perfect monograph of Dr. William Budd, "On Typhoid Fever," in the whole of which there is not a syllable on the

subject of medical treatment, the entire treatise being solely occupied with discussions on the cause of typhoid fever, the pathology illustrative of that cause, the modes of propagation of the specific poison, and the methods of prophylaxis which a knowledge of those modes indicates. Even Dr. Murchison would, on the like plea, be censurable for introducing what he himself emphatically points out as a "feature unusual in a practical work in the large share of attention he has devoted to the Causes of Fevers." But he has done so from a "conviction that Continued Fevers are diseases which may be prevented, and the circumstance that the questions discussed have occupied greatly the attention of scientific men of late years." Thus he hopes to stimulate other observers to further investigations, to elucidate the causes of "some of the greatest calamities which have befallen our race," and so help to "prevent similar calamities in future." It can be no sin to share in his zeal, nor to endeavour to follow his great example.

As having an immediate and interesting bearing upon the subject of the remarks at

page 118 on the method to be pursued in the future experimental investigation of the cause of typhoid fever, the following account by Dr. E. Klein, the able coadjutor of Dr. Burdon-Sanderson, and joint author of the celebrated "*Handbook for the Pathological Laboratory*," are here introduced. The account of this important research was only received immediately after the part where the topic is referred to was in print. An account of Dr. Klein's observations on the small-pox of sheep appears in the "*Proceedings of the Royal Society*" of June last.

"KLEIN ON THE PATHOLOGY OF ENTERIC FEVER.—Dr. E. Klein has for some time past been engaged, at the instance of the Medical Department of the Privy Council, investigating the morbid anatomy of typhoid fever by means of the microscope. As this inquiry is temporarily suspended, he has communicated an abstract of the results to the *Centralblatt für Medicinischen Wissenschaften*, for Sept. 21 (No. 44), from which we take the following.

"1. Sections (previously hardened) of the ileum from persons dying in enteric fever, show that an active (*rege*) absorption of peculiar organisms has previously occurred in the mucous membrane over Peyer's patches, especially in that just surrounding them; and that these are carried thither by the lymphatic vessels and veins of the mucous membrane.

"2. The earliest case examined by him (dying seven days after the first occurrence of headache) exhibited peculiar greenish brown, for the most part spherical bodies, which were found within (*im Lumen*) the follicles of Lieberkühn; the size of these was somewhat variable, the largest being two or three times as large as a red blood-disc, whilst the smaller were half or quarter of that size. They lay for the most part massed in groups, presenting an olive-green tint. At the edges of such groups, or where they lay singly, many sectional outlines were visible—reniform, biscuit-shaped, etc. These minute bodies were also found in the mucous tissue, and appeared to be contained within the lymphoid cells (lymph corpuscles) of the adenoid tissue. The veins and some of the lymphatic spaces were very rich in these organisms. In the former, they soon proceeded to exhibit a rapid double and quadruple fission, thus splitting off into still more minute, and for the most part greenish yellow, granular micrococci; which combined together in twos, or fours, or in necklace (rosary like) chains, or even actual zooglœa. The micrococci, moreover, were closely related to a mycelium with branching threads, smooth outline, and a greenish-yellow colour. [These results are similar to those which Dr. Klein communicated to the Royal Society as regards sheep-pox—the account of which is now in the press.] These organisms are not only found close to those Peyer's patches which are moderately swollen, but also in parts of the mucous membrane which only exhibit very slight general swelling, when viewed with the naked eye. The Peyer's patches in such parts exhibit peculiar changes which will be described hereafter. In addition to these organisms, one finds masses (*Ballen*) of greenish-yellow micrococci pervading the mucous membrane from the free surface, the epithelium, and in the tissue itself, especially in

Lieberkühn's follicles, and passing from these again into the lymphatic canals. [As Dr. Klein promises a continuation, any comments would be premature.—*Rep.*]—*Lond. Med. Rec.*, Oct. 1874.

It was no part of my design to deal with points of treatment, medicinal or sanative, details of which abound in able professional text-books, sensational popular manuals, æsthetic serials, practical journals, and reports of boards of health, often written in fastidious taste on the ever unwelcome topic of public hygiene.

My only duty has been to recapitulate the general laws of Typhoid Fever, and, by applying them to the special case of this new country, teach the Central Board of Health how to explain to the people, what it has never been able to explain to them before, the ways the fever has of wandering over the country, through every community.

In the matter of medical treatment, therefore, always the most genial study of ordinary medical minds, and grateful to the presently sick, yet of interest inferior to the grander laws of causation, it must now suffice if I show how one good illustration of the effect of arresting

specific poison emanating from fevered bodies is gained by a practice often adopted in scarlatina. On many occasions this fever has been stopped from passing beyond a single child in large families, when that child had brought fever, say, from an affected school, by sponging the body night and morning with warm water and mustard, drying and oiling with warmed salad oil, and placing it in clean bed and body linen. This process carried out morning and evening, the soiled bed and body linen carefully rolled up, without shaking it about the bedroom, placed in a pail of cold water by the bedside, the water-tap turned into the pail for a while, the clothes hung out to dry, and then washed in the usual way, has never failed me in a single one out of many trials. Before adopting this method I had always taken for granted that scarlatina once brought into a family of children must pass through them all; that if any of them were sent away they would surely take it on return, no matter what pains were taken to clean and disinfect. Now, however, I feel just as certain that scarlet fever need never go

beyond the introducer of it into a family or a school. The same principle applied to typhoid would equally succeed, the process of removal being merely modified according to the different surface whence the poison is cast off. In this process no kind of disinfectant whatever need be used. Trust in the simple action of cleanliness and pure air for quick removal of all exuvia is alone required. Strong deodorants are superfluous and therefore hurtful. The use of oil mechanically arrests the scales from the skin, combines with the animal matter, and, further, is absorbed to supply fat to the invalid, the necessity of which was well shown by Professor Parkes,¹ in treating of the elimination of nitrogen in fever.

Having disposed of this element of preventive treatment of typhoid fever, I shall briefly confute an argument for one of its assumed causes, by pointing out the self-stultifying action of the Central Board of Health.

In the above report on Typhoid Fever at Sunbury Schools, the exposure of fæcal matter

on the bare ground is an alleged cause of fever. "The contents of closets were thrown out only a few yards from the house"—on the midden at the door. Before the fever came, "*and for years before*, the contents of the earth closets were carted away and deposited within a few hundred yards of the buildings. An offensive smell was felt from these deposits by the sick man, when he returned convalescent. The unpleasant smell was no doubt there before he took ill; but he did not pay that attention to it which he afterwards did when he learned the probable pythogenic origin of this kind of fever. There were others also who felt the offensive effluvia from the decaying fæcal matter of the closets. *It was subsequently carted away by order of the Chief Medical Officer.*" Fortunate Sunbury! Melbourne's olfactories are cared for only by the Central Board of Health. "The stools from the fever cases were disinfected with carbolic acid, at least for part of the time. By part of the time, I mean during the worst part of the fever; for probably about the commencement, and after the crisis, this precaution might not have been adopted. I

also gave instructions that the stools were not to be thrown into the closets, but taken away and buried. I now learn that covering the stools with earth was not always carried out. This work was done by one of the boys in the hospital, and it seems that the stools were taken away with the carbolic acid, and thrown on the ground without being always covered with earth. For many years the contents of the closets have been carted away, and deposited on the surface of the ground within a few hundred yards of the buildings. Of course, the earth closet system has a great advantage in neutralizing the injurious effects of the fæcal matter; but when exposed to the weather, the rain washes away the earth, and leaves the animal matter more or less uncovered in the upper strata. This is now being removed to a proper distance, as also the emptyings from the closets."

"Carted away by order of the Chief Medical Officer." Why? Lest it should create fever amongst people who are compelled to inhale the effluvia? If an effluvium was by the Chief Medical Officer thought capable of becoming a

predisposing or an exciting cause of typhoid fever at Sunbury, upon what grounds does he refuse to deem it such when acting upon the residents of the central city? People living in and around Melbourne have long suffered from like inflictions, and have in answer to protests been assured that their fears were unfounded. Yet, if imperfectly covered fæcal matter, or that laid bare by rains, caused fever far in the country, in an isolated school community of slovenly-kept prisoners, whose "urine was likewise deposited too near the wards; children who did not strictly confine themselves to using the closets;" *a fortiori*, the like causes should have been even more virulent in the larger dense community where they more abound. Between the two communities there is only this difference, that whereas at Sunbury the offensive smells arose from reeking rottenness from wanton dirt, at Melbourne the effluvia come from fermenting fæcal matter, cheaply got rid of in the twofold economy of cleansing the town and beautifying the gardens of its environs:—

"Where Nature only suffers in disgrace."

But the difference of motive cannot alter the effect. If fæcal exposures cause fevers at Sunbury, so must they at Melbourne. The offence was "*ordered*" to be at once removed at the former place; at the latter it was equally peremptorily "*ordered*" to be continued. The prisoners' complaint was heeded; that of the citizens laughed at—like the wail of a chain-gang on a felon ship. But whether it caused Typhoid Fever, or only some other more insidious but as deadly undermining of general health, rests with science to decide.

In revising my report for the purpose already plainly avowed, I have endeavoured to adapt it to the wider audience now addressed; and, while not sacrificing the technical part of the subject through any desire to be exceeding lucid for popular perusal, I hope that laymen may not find it hard to follow the argument. The impossible feat of writing a popular treatise on fever, to make experts for self-physicking, has not been attempted, but only so much as may help to convey a clear idea of the true cause of typhoid fever. Popular works on science affect us, says Pettenkoffer, "like

listening to good music ;” for we may hear
Jenny Claus

“ Untwisting all the chains that tie
The hidden soul of harmony,”

without ourselves trying to emulate the performance. Nor need “the Muse be dumb though Apollo lecture” to make tune more enjoyable without a headache ; or to raise “public sympathy for the work of those men whose calling requires them to enter more minutely into those subjects”—the only allowable aim of popular medical writing. Therefore it is, too, that in this revision the copious literature of fever has been sparingly referred to. Indeed, free use of it would in this town be a rather difficult matter, the supply of the commodity being somewhat scant in Melbourne. The collective profession has brought together a few odd volumes in its little bibliothéke ; there is the nucleus of a medical library in an outer shed at the University, divorcing medicine from the humanities, to make it all ‘practical’ and nothing literary ; while a fair beginning of a medical section of the Public Library is placed on an over-heated platform where the pursuit

of knowledge broils in difficulty. Melbourne architects should mount that fiery reading-spit, and late o' nights study the relation of oxygen to animal life, until they gave lungs to public buildings. With fresher air there and more books, my citations might have been more numerous, with greater brevity.

To write of fever in the home of the far-famed "*fever tree*," without adding a word upon its merits, would look like denying it honour in its own country ; and to avoid that appearance of ingratitude, a leaf will now be gleaned from indigenous blue-gum literature. Belief in the fever-preventing power of the tree spreads out like its own knarry roots. The valetudinarian who planted a specimen in a flower-pot in his conservatory, through no abracaxic faith in the charm of a phylactery, but in utilising the chemical action that purifies the air, will find the root that in the *habitat* covers an acre, rather "constricted ;" but his potted blue-gum will now become emblematic of popular medical superstition, if, by creating false hope of security, it prove not rather more *Upas* than fever-tree.

At the Royal Society of Victoria a paper was lately read on the "*Fever-preventing power of the Eucalipti*;" and the amount of empyreumatic oil, acid, and essence exhaled into the atmosphere elaborately calculated. To their ozone-forming force was ascribed the assumed immunity of Australians from fever. Whether that freedom was real or imaginary had not been first of all determined; for with all the strong aromas that were said to repel fever, by still more pungent odours of other ozone generators have fevers to be driven away.

Alluding to the various ideas about the cause of the autumnal increase of fever, the author of the paper propounded the notion that it arose from a lowered amount of active ozone in the air, owing to a protracted summer and autumnal drought causing lessened exudation from the leaves of the parched gum trees, observing that:—

"As evidence on this part, let me refer to the circular issued last May by the Central Board of Health to the medical profession, inviting 'further information respecting the

continued fevers now prevailing in and around Melbourne.'

"The following is one of a list of questions forwarded :—

" ' 7. Have seasonal peculiarities influenced the fevers ? ' "

"Speaking as one outside of that profession, the following may be stated—That the leading oil-producing species of the *eucalypti* were, during the season prior to last May, extra poor in volatile oil, and if any connection exists between the amount of ozone in the atmosphere and the occurrence of epidemic or other forms of disease, we have given ample evidence that large quantities of the oxidising agents usually known to exist in Victorian air during that season were absent."

This accepted exposition was followed by the query:—"Is the Eucalyptus a fever-destroying tree?" and authoritatively answered thus :—

"The various fever types as found existing amongst us at times appear malignant, arising either from importation or from the existence of bad sanitary regulations; but

medical testimony is that virulence is meteor-like, 'dies at its opening day.' No credit can be taken for any improved sanitary condition of our surroundings by ourselves in our towns and cities, the influences operating there *entice* the poison fever germ to fructify and abound."

The same dry hot air that hastens the withering of typhous particles robs the gum tree roots of genial oil-forming moisture, and this lessened supply of ozone-creating oil is the cause of typhoid fever! Why should not we as well assert that the increased fever was the cause of the diminished ozone? It would be equally good logic. The fallacy affords a capital illustration of the usual *a priori* mode of deductive argument adopted whenever physicists analyse fever etiology. It is only a few months since Sir William Hooker informed the House of Commons that the febrifuge virtues of the gum tree were greatly exaggerated, a delicious euphemism for delusion. Indeed, it would be about the very moment of their being so highly extolled here; even while an inquiry into our experience of con-

tinued fevers in Victoria enabled us to declare that the quality then engrossing the minds of our philosophers was pure myth. "An' please your majesty, I doubt the fact. An' please you, so do I."

Finally, this report has been disparagingly contrasted with that by Dr. Ballard, on a localised outbreak of Typhoid Fever at Islington, in 1870. But the two reports cannot be justly compared. Concurring in all the praise bestowed upon Dr. Ballard's able work, so strangely brought into rivalry with mine, I yet deny that a parallel can be drawn between them. The two fields of inquiry were quite unlike. At Islington fever was confined to a narrow spot, the cases were readily found, and known to be true typhoid, without doubt of diagnosis. Medical men attending, animated by united will to find the truth, freely interchanged intelligence; and when the clue was given by an outsider, the deadly agency at work was not hard to unearth. The very milkman vied with medicine in the cause. But here, on the contrary, was a broad field of ten

miles radius to hunt over, for many modes of catching fever, with sick people living in varied ways of life, all differently attacked; with medical men and milkmen reserved, reticent, or even ridiculing the chase after a cause as a sort of unprofessional poaching on the manor of death. "It was by medical men talking together" that the full extent of the Islington outbreak became known. But here all were as close as if every one held in confidence a fatal secret that an emissary from a secret police treasonably tried to extort. If the questions had been the inquiries of a political spy, my inquiries could not have been more heartily scorned. Even the president of the Board of Health held aloof in sulky silence, dreading the machinations of some soulless automaton this Frankenstein himself created, unfit for typhoid sympathy, and watching a chance to break in bits the fragmentary monster. It is averred that leaving me thus severely alone was letting me be free and unfettered to form a perfectly independent opinion; but the freedom was as negative as the bondage, for

the bricks had to be made without the proverbial straw.

Again, Dr. Ballard nearly fell into a great fallacy. He had come to the belief that the fever arose from bad drains. So assured he rested, and closed his search at a point where I too could have stopped mine, and with equal *eclat* given a fresh proof of pythogenesis. He found plenty of offensive effluvia in living-rooms, fetid gases from rat-burrows, foul cow-yards, and offensive cesspools. "So frequently were such or similar conditions met with in my inspection that, as any one else probably would have done, I attributed the typhoid to the sources of miasm I discovered, and for a time was quite satisfied that I was correct." So have I likewise found foul conditions abounding, and might have come to the same conclusion.

But Dr. Ballard was rescued from that serious blunder by the keener perceptions of a woman, who quickly discerned the real source of mischief, hinted it to her own medical friend, who in turn told Dr. Ballard. She had changed her milkman, and, so like

an acute busy-body, induced four or five families of her acquaintance to follow her example, and they forthwith all fell into fever together. It was this extraordinary simultaneousness that arrested everybody's attention; for at first Dr. Ballard was sceptical; thought it a coincidence, curious but of no scientific value; and did not hesitate to check gossip about milk by saying so. But the scandal against the Brinvilliersian pails grew to alarm so great that Dr. Ballard was compelled to re-open his inquiry, and following the clue so given found out the offending dairy.

But even after tracing up the fever to the *fons et origo mali*, it still was left dubious whether the foul water in the tanks in the cow-yard, used to wash the milk-cans, was also added to the milk, or only contaminated with common sewage, or fæcal matter, or fæcal stuff holding grains of fever-poison. Therefore, exact as was the logic of the argument, it did not determine one iota of the disputed question of autogenesis or of contagion. As far as Dr. Ballard's inquiry went, these problems were left unsolved; indeed into their

decision he hardly tried to enter, and hesitated at the last with a conjecture.

But here, on the contrary, was not I required separately to determine the following propositions? viz.:—

1st. Whether the prevalent fever was or was not true typhoid? This necessitated a discussion of the intricate questions of diagnosis and classification, such as was not required of Dr. Ballard.

2nd. Whether the fever was or was not more prevalent last autumn than at corresponding seasons of former years? demanding minute collating of statistics, such as was not called for from Dr. Ballard.

3rd. Whether the fever was more or less severe or fatal than formerly? again requiring copious statistical data, such as Dr. Ballard was never asked to furnish.

4th. Whether any conditions common to the widely-different localities suggested a common cause of fever? while Dr. Ballard needed only to search for

a cause common to one spot of very limited boundaries.

5th. Whether any peculiarity amongst persons attacked touched the cause? and on this point alone was there an object of pursuit similar in both inquiries. But Dr. Ballard found only one medium of contagion, while I find many modes.

6th. Whether there were proofs of contagion? a point which Dr. Ballard barely touched, but which became imperative on me if possible to exhaust.

7th. Whether seasonal peculiarities influenced fever? calling from me an opinion on the most abstruse subject in fever history, and an elaborate analysis of meteorological phenomena in reference to causation, the like of which was never expected of Dr. Ballard.

8th. Whether domestic customs in mode of milk or water supply helped to spread fever? a point that was formally set to me, but which Dr. Ballard had forced upon him accidentally.

In connection with this last part of the category of causes, I have clearly shown two instances wherein more than a suspicion rests on milk as the vehicle of carrying contagion, because in each case the fever followed the importation of fever into the dairy; whereas in Dr. Ballard's case he failed to show how that the blamed foul tank-water had been long used without creating fever, which did not appear until *after* the milkman's family were taken ill. But how they became ill was left by Dr. Ballard to future conjecture; because it was not shown whether they were self-poisoned by drinking their own milk, or had caught fever from an outside source of contagion, and then affected their own milk, which in regular sequence poisoned their customers who drank it, as seems to me to have been the more probable order of causation. "None of the men exposed to emanations had fever except those who had been in the way of drinking the milk."¹ This fact deserves a few remarks.

¹ *Medical Times and Gazette*, Nov. 26, 1870.

The foul tank water had long been used without creating fever, which was unknown until a girl came home from service, ill of typhoid fever, to her mother's house at the dairy. Her's was the first case. From this imported case the fever spread probably in this wise:—The mother who nursed her by night and day, also milked the cows. With the usual habits of her class, she would likely go from the sick bedside to the cows without changing garb, or, perhaps, without even washing hands; and from the frowzy person contagion particles would drop into the milk pail. To suppose so precarious a chance of conveying contagion could be efficient may seem imaginary, the small amount possible so to transfer being hardly conceivable as adequate. But, an infinitesimal speck is enough. A minute drop of vaccine virus suffices for vaccination; when it dries it is still virulent. Equally so is typhous virus. Possibly one microzyme would inoculate. That little organism is about one 20,000th of an inch through. The finest line the fancy could spin might string twenty thousand such

beads on every inch. An epithelial scale from the diseased bowel carries a small colony. If a milkmaid who is also a fever nurse were viewed while lit up by the electric light, she would shine at her cow's side in a halo of fever-dust ; the particles falling all around ; and, some of them into the milkpail. What happened at the dairies near Melbourne, probably occurred at Islington, although the conjecture is left out of Dr. Ballard's otherwise able report. It is certain that the girl who came home from service sick was the first case of fever in the dairy ; that the mother nursed her, and milked the cows ; that the dairyman next took ill ; that he too was nursed by those who likewise milked the cows ; and that the rest that follows is very cogent scientific induction. I may be reminded that this girl used milk from the dairy while she was at service ; but, still, she had been at home *within* the dairy for a fortnight before any one else about the dairy was seized with fever ; before in fact any one else who drank the milk, was ill. Further, it is not shown that this girl had not been exposed to con-

tagion at the place where she took ill, that is, where she resided before returning home sick to her mother's. The evidence, therefore, appears conclusive that it was she who introduced fever into the dairy, and distributed it among the customers; and if the fact had been indicated by Dr. Ballard, it would then have placed the cope-stone upon the edifice that he had otherwise built up so fair. He records the fact, but omits the logical inference.

Some few years ago I thus explained the process of milk contamination to a gentleman whose family suffered much from scarlatina, for which he blamed the drains. The source of contagion was evident to me, because I visited the milkman's family, and saw the operation of contagion going on. My friend, incredulous, consulted others, who advised him "to pull down his house and rebuild;" but the sage counsel was not followed, yet there has been no more fever. I traced a similar scattering of contagion by a greengrocer, who kept the matter quiet in case it might injure his trade. There too foul drains were the stalking-horse.

During a diademic of imported small-pox, one fatal case could not be followed up to a contagion source, and it was called "sporadic." The secret of the direct source of contagion was confided to me. The disease was at first *mis-*diagnosed by four of our busiest physicians as "*only blood-poisoning*," and the life thereby lost. I had silently to look over that error, and the Board of Health's still grosser blunder of *mal-*observation: that, because they failed to trace the parent case, therefore the fever was of spontaneous origin, from occult "atmospheric or telluric conditions."

That the medical mind should so fondly yearn after esoteric influences when exoteric causes stare them in the face, must surely be owing to some mentally-framed relic of the Cimmerian ages of medicine, when fevers were held due to diabolic entities that for a time had taken up their abode within the body, to be driven out only by drastic drugs, or exorcised by the read gospels of a devout clerk.

Within the last few days the following short narrative has been kindly sent to me by a

very young lady, who was personally cognizant of what she so precisely relates:—

“To show an instance how the disease (*Diphtheria*) may be conveyed from place to place, allow me to relate the following circumstances:—

“Four children in a family at Terang were attacked with true diphtheria, and all recovered. While the fourth was getting better, it was taken to see an aunt at Darlington (where the disease had never been heard of), who had a large family. Shortly after the child arrived her family were all taken ill with bad sore throats, it was said; so, thus put off their guard, no one shunned them. The consequence was that the children mixing freely spread the disease in all directions. The next family attacked was that of a hotelkeeper, who likewise reported the disease to be ‘*only sore throat*,’ as was very natural; because Christmas was drawing near, and if it became known that his family had so terrible a complaint amongst them as diphtheria, his trade for the holidays would be injured. Not until a young man aged 20, from a neighbouring station,

visiting the hotelkeeper's family, and taking ill, did the true nature of the malady in the village transpire, and then only after the young man's little brother aged six years, was attacked, and died, the medical man saying 'it was only putrid sore throat,' until immediately before death, for both cases proved fatal, the boys dying within a few hours of each other."

If all young ladies longing after the '*higher education*' would employ their observing talents in these socially interesting problems, they would cultivate their minds to some useful purpose, as logically as by the more captivating competition studies. It is perhaps to our future medically trained mothers that after all we shall have to look for a clear account of contagion facts; for we despair of men's metaphysical entanglements. Mothers have to face the battle, and cannot all run away from the great invading foes of the nursery, fleeing like our nursling grenadiers to the wilds of Caulfield for a new "*Decamerone*."

Finally, I may safely assert, that for the first time in the history of the Australian colonies has now the causation of typhoid

fever been systematically investigated from accurate data. I state this with all respect for the labours of others who have endeavoured to deduce the laws of zymotic disease generally from theoretical reasonings; and in all admiration of a few excellent essays on practical questions connected with the more directly medical treatment of fever. But for an effort to elucidate the cause of typhoid fever from its ascertained modes, I think this is the first attempt yet made here, and therefore it may well be excusably imperfect.

I have had to write with some versatility upon intricately varied yet closely interwoven themes not of my own choosing; but prescribed for me by those who find fault with my discursive discussion. Yet why should I complain of this criticism, when these very critics find fault with their own act in setting this inquiry afoot, deprecate needless alarm, and try to lull people's fears by assurances of there being little to speak about? They will probably be upheld in this opinion by the experience of the coming autumn; for, if the inductions from bygone years be fair, the

present open showery summer will check the action of contagion, and lower fever below the average; while there will be ample heat and moisture to favour Pettenkofer's theory of subsoil fermentation, and prove or disprove its truth.

Such considerations have apparently little interest for the majority of medical men. At this moment measles are running a wild raid over these colonies, through eight or nine out of every ten families amongst us, disbanding schools, making offices short-handed, and driving the very military away from the barracks to camp in country quarters; but how the disease came very little concerns the Faculty, judged by public utterances; for the great oration of its annual congress avoided the topic as wholly beneath the speculations of the medical philosopher.

The same remarks apply to two recent *diademics* of *Cynanche Parotidæa*, and of *Rubeola Notha*, so often mistaken for true measles in deciding the oft put query, Can measles be had twice? Outbreaks of these and other fevers have come and gone, but left no record.

Australian fields are either sterile wastes, or virgin soils for epidemiological culture.

Trusting that in this humble effort to aid "the universal diffusion of common means of decency and health" I may venture to seek an audience beyond the "petty boards" that "let loose Fever and Consumption" to "keep their little fiddles going, for a Dance of Death;" and, having begun with two great masters of Typhoid causation for exemplars, dare in closing to repeat the words of a third, by adding, that "where the endeavour has been to write not one useless word, but to seek alone precision, the form may be found too severe. But if with this fault some friends of truth will judge it useful to science, I will find in their testimony the recompense of a work *assez pénible pour qu'il soit difficile de s'en faire une juste idée.*"

FINIS

ADDENDUM.

The Author of this Report on Typhoid Fever is to some extent gratified by the fact, that, although the Central Board of Health say, in their official commentaries upon his report, that they are unable wholly to agree with the contagion theory advocated, they yet have nevertheless adopted his practical suggestions, so far as to have issued, in their first stage of persuasion, the following Circular to all medical men and local boards of health in the colony. Time and further study will doubtless bring conviction.

“ [CIRCULAR.]

“ CENTRAL BOARD OF HEALTH,

“ Melbourne, 20th January, 1875.

“ MEMORANDUM.

“ Adverting to the inquiry which the Central Board
 “ of Health recently caused to be made with respect to the
 “ prevalence of typhoid fever in this colony, the result of
 “ which inquiry is published in the Board’s Fifteenth
 “ Report, the Board now desire to draw the attention of
 “ the medical practitioners of the colony to the subject, and

“to invoke the aid and cordial co-operation of the profession in carrying out certain measures which are therein suggested for preventing the spread of the disease.

“It will be seen by the Report in question that, while the Board do not consider that the inquiry referred to has conclusively established the truth of the theory that typhoid fever is solely spread or kept alive by the alvine discharges of the patients, they are nevertheless of opinion that enough has been shown to render it the duty of the sanitary authorities to endeavour to have carried out those precautions which would be indicated as necessary were the correctness of the theory actually placed beyond doubt. Those precautions appear to the Central Board of Health to consist in (1) the isolation of the patients; (2) the immediate and thorough disinfection of their alvine evacuations before these are cast into any cesspool or closet-pan, or otherwise disposed of; and (3) the immediate disinfection, by boiling water, of any bed or body linen or other articles which may be soiled by the patient's discharges—these measures of disinfection being continued as far as practicable during the convalescence of the patients and until recovery is established.

“The Central Board have accordingly addressed the various Local Boards of Health urging them to do all that lies in their power to secure the carrying out of the suggested precautions wherever typhoid fever may be present, and they now appeal to the various members of the medical profession in the colony, whatever their individual opinions may be as to the causation or propagation of this disease, to lend their best assistance in giving these precautionary measures a fair trial.

“At the same time, and with a view to a further elucidation of the important question of the causation and propagation of typhoid fever, the Central Board of Health

“will feel obliged if the members of the profession will
“communicate to them such particulars of any cases of
“the disease which may come under their care during the
“next twelve months as will tend to throw light on the
“point whether the disease has arisen from contagion and
“in what manner,

“The importance of the question to the future sanitary
“welfare of the colony, and the known desire of the
“medical profession generally to further its sanitary
“interests, will, the Central Board of Health trust, be
“sufficient excuse for their making the present appeal.

“(By Order of the Board),

“T. R. WILSON,

“Secretary.”

By the Same Author

ON

PHTHISIS

AND THE

SUPPOSED INFLUENCE OF CLIMATE

BEING AN

*Analysis of Statistics of Consumption in this
part of Australia*

WITH REMARKS ON THE CAUSES OF

THE INCREASE OF THAT DISEASE IN MELBOURNE.

E R R A T A.

Page	39,	line	17,	for	"bring,"	read	"bring out."
"	70,	"	4,	"	"opinion,"	"	"opinions."
"	83,	"	2,	"	"Tyndal,"	"	"Tyndall."
"	84,	"	Note,	"	" <i>Arrat.</i> "	"	" <i>Anat.</i> "
"	113,	"	15,	"	"fætid,"	"	"fœtid."
"	123,	"	11,	"	"Helmholz,"	"	"Helmholtz."
"	150,	"	11,	"	"Albumenoid,"	"	"Albuminoid."
"	193,	"	25,	"	"Pettenkoffer,"	"	"Pettenkofer."
"	196,	"	7,	"	"Eucalipti,"	"	"Eucalypti."
"	214,	"	9,	"	"began,"	"	"begun."

